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Cover illustration: Director Oliver Stone confers with Kevin Costner on the set of *JFK* (1991). © Warner Brothers, Inc. See the three-part *AHR Forum* in this issue beginning on p. 486.



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In This Issue

Linda K. Kerber examines the gendering of the political discourse of citizenship in American history. In the case of *Martin vs. Massachusetts*, tried in 1805, the son of loyalists succeeded in winning the return of properties confiscated from his mother on the grounds that a married woman was not obliged to display patriotic loyalty if it placed her in political conflict with her husband. In opposition to this conservative stance, the state's attorneys advanced an unusual and radical interpretation of revolutionary era thought, arguing that married women were full members of the body politic and had the responsibility to assume the obligations of citizenship. This argument demonstrates a break with the past and the ability of the founding generation to conceive of an alternative to coverture and patriarchy.

Sarah Deutsch looks at three middle-class women's organizations in Boston and their evolution from the nineteenth into the twentieth centuries, focusing on the timing and causes for the organizations' inclusion of men and the practical and ideological effects of the participation of men. Deutsch contends that women's politics changed as the city changed and interacted with Progressive urban politics in ways that have not been discussed before.

In our first *AHR Forum* section of this issue, **Russell Jacoby** applauds the recent rethinking of intellectual history. In discussing the work of Hayden White, Dominick LaCapra, and other leaders of the renaissance of historical theorizing, Jacoby also points out what he sees as an irony and failure of their work. Their concern for language moves them toward dogmatic relativism and formalism, the latter reflected in their penchant for a technical, almost scientific vocabulary, devoid of literary quality. Their effort to return to the literary roots of history turns out, therefore, to be neither new nor very literary. **Dominick LaCapra** responds with a rejection of the composite image of a movement Jacoby labels "a new intellectual history," an amalgam that ignores significant differences among the figures discussed. LaCapra sees the danger in Jacoby's approach as its tendency to foster a convergence between "leftist" criticism (which Jacoby espouses) and neoconservative initiatives by castigating intellectuals in the academy and fabricating a myth of a lost golden age of more socially responsible public intellectuals. A focal point of this attack on intellectuals in the academy is a stereotypical idea of "theory" as a bloodless, abstract diversion from "real" social and political practice, a stereotype that LaCapra counters by reexamining the relations between history and theory and discussing their fruitful interaction.

Molly Myerowitz Levine provides a thorough orientation in the issues and criticism associated with the first volume of Martin Bernal's *Black Athena: The Afroasiatic Roots of Classical Civilization*. Levine praises Bernal's ability to provoke a major rethinking of classical history, but she also takes him to task for not defending his study against misuse by some Afrocentrist writers. **Robert L. Pounder**, a specialist on bronze age archaeology, reviews Volume 2 of *Black Athena* and notes the failure of its author to treat archaeological evidence with appropriate rigor. He sees Bernal's construction of classical periodization and influences as fanciful efforts to justify a preconceived model.

Janet J. Ewald reviews seven recent works on slavery within Africa and the slave trades emanating from Africa. She explores the problem of finding a common framework for comparing the Atlantic slave trade, and the routes taking slaves across the Sahara Desert, Indian Ocean, and Red Sea, as well as slavery within sub-Saharan Africa. In addressing Atlantic slavery and slavery on the African Atlantic coast, the books under review share a common and well-established set of questions, but no equivalent central questions dominate scholarly inquiry into Africa's slave trade with the Islamic world and slavery within Islamic Africa. Ewald does find in these books a start toward building a workable comparative approach to the various slave trades.

The recent film by Oliver Stone, *JFK*, is the subject of our second *AHR Forum*. **Marcus Raskin** evaluates the film as a powerful counter-myth to the myth of the Warren Commission report and points to the unpleasant fact that "conspiracies," or joint action for a political goal, are the daily practice of those who serve in government. **Michael Rogin** sees the film as more than the traditional American conspiracy theory. Stone's film is part of the psychological reaction to an unresolved loss, the assassinated president. **Robert A. Rosenstone** rounds off the *Forum* with a discussion of the problems and possibilities of portraying history on film, how the medium places requirements on the representation of the past different from those used in prose constructions. Historical films must be judged by their effectiveness in raising issues, and on this score Rosenstone rates *JFK* highly.

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Contributors

Sarah Deutsch is an associate professor of history at Clark University. She is currently investigating the way in which the geography of the city affected and was affected by the strategies and opportunities provided by class, race, and gender relations. The article in this issue forms one chapter of her larger study of space, power, and gender in Boston from 1870 to 1950, to be published by Oxford University Press. Some of these issues arose in her earlier work, *No Separate Refuge: Culture, Class, and Gender on an Anglo-Hispanic Frontier in the American Southwest, 1880–1940* (1987).

Janet J. Ewald, who received her doctorate from the University of Wisconsin, is an associate professor of history at Duke University. In her book, *Soldiers, Traders, and Slaves: State Formation and Economic Transformation in the Greater Nile Valley, 1700–1885* (1990), she analyzed how the Taqali kingdom, located on the Islamic frontier, developed from both local and regional dynamics. Central to those dynamics was slave raiding, trading, and exploitation. Currently, Ewald is examining slavery in the lands bordering the Red Sea, specifically, how slavery and the slave trade continued in an age of supposed modernity and how local Muslims and Western observers together constructed an image of Islamic slavery.

Russell Jacoby is currently Visiting Associate Professor in the Department of History at the University of California, Los Angeles. He has written *The Last Intellectuals: American Culture in the Age of Academe* (1987), *The Repression of Psychoanalysis: Otto Fenichel and the Political Freudians* (1983), several other books, and numerous reviews and essays. He attended the University of Chicago, the University of Wisconsin, and the University of Rochester, where he received his doctorate, working with Christopher Lasch and Georg G. Iggers. Jacoby maintains an interest in both American and European twentieth-century intellectual history; he is writing a book on American higher education in the twentieth century.

Linda K. Kerber is May Brodbeck Professor in the Liberal Arts and Professor of History at the University of Iowa and currently Visiting Professor of History at the University of Chicago. The author of *Women of the Republic: Intellect and Ideology in Revolutionary America* (1980, 1990) and *Federalists in Dissent: Imagery and Ideology in Jeffersonian America*

(1970), she co-edited, with Jane Sherron De Hart, *Women's America: Refocusing the Past* (3d edition, 1991). Her book in progress, of which this essay is a part, is a much-expanded version of the Jefferson Memorial Lectures presented at the University of California, Berkeley, in 1989, and will explore the gendered and paradoxical character of civic obligation in the United States from the American Revolution to the present; other sections focus on jury service and military obligation. Kerber was a student of Annette Baxter at Barnard College and of Richard Hofstadter at Columbia University.

Dominick LaCapra is Goldwin Smith Professor of European Intellectual History at Cornell University, where he has taught since 1969. His books include *Rethinking Intellectual History: Texts, Contexts, Language* (1983), *History and Criticism* (1985), and *Soundings in Critical Theory* (1989). He has co-edited *Modern European Intellectual History: Reappraisals and New Perspectives* (1982) and edited *The Bounds of Race: Perspectives on Hegemony and Resistance* (1991). He received his doctorate from Harvard University in 1970. His interests are cross-disciplinary and bear on the relations of history with literary criticism, philosophy, social theory, and psychoanalysis. LaCapra currently is at work on a collection of essays addressing the problem of how to integrate history and theory.

An associate professor of classics at Howard University, **Molly Myerowitz Levine** was formerly at Bar-Ilan University, Ramat-Gan, Israel, where she earned her doctorate. Although once a *litterateur* (*Ovid's Games of Love*, 1985), she has been interested in the sociology of knowledge since she began teaching and studying myth. Trained at Harvard and Yale, she organized the American Philological Association presidential panel that first brought *Black Athena* before the classics establishment. The papers from the panel were subsequently expanded and published as *The Challenge of "Black Athena,"* a special issue of *Arethusa*. She has also found connections to Martin Bernal's work in her scholarship—a forthcoming book on ancient Mediterranean hair codes—and in her teaching—a collaborative project by scholars engaged in rewriting the Classical Civilization course as an Ancient Mediterranean course, to better reflect the cultural interchange, borrowings, and parallel developments between Greece and Rome, Egypt and the Near East.

Robert L. Pounder received his undergraduate education at the University of Alberta, where he was granted the B.A. (honors) degree in classics in 1964. He studied with Robert J. Buck and George Hardy, among others. His graduate work was in classics at Brown University. Alan L. Boegehold was the adviser, with Sterling Dow of Harvard as an unofficial mentor, for his doctoral dissertation, "The Origin and Meaning of *Theoi* in Greek Inscription Headings." Currently Assistant to the President and Professor of Classics (since 1972) at Vassar College, he is the author of "Honors for Antioch of the Chrysaoreans," *Hesperia*, 47 (1978); "A Hellenistic Arsenal in Athens," *Hesperia*, 52 (1983); and "The Origin and Meaning of *Theoi* as Inscription-Heading," in *Studies in Honor of Sterling Dow: Greek, Roman and Byzantine Studies*, Monograph 10 (1984), among other articles and reviews, chiefly for the *American Journal of Archaeology*. He is at present preparing for publication Hellenistic inscriptions from the excavations on Samothrace.

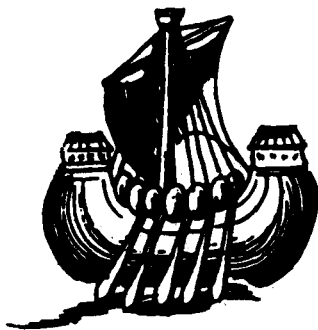
Marcus Raskin is Distinguished Fellow and co-founder of the Institute for Policy Studies. The author of a number of books on political theory, government, and national security, he is the author of *Being and Doing* (1971), *The Common Good* (1987), *New Ways of Knowing* (with Herbert Bernstein) (1987), *Essays of a Citizen* (1991), as well as books on the Vietnam War and the National Security State, a term he coined. He has a deep interest in the Kennedy assassination and that period in American history, having served as a member of President Kennedy's National Security Council staff. Raskin is completing a second vol-

ume of essays provisionally titled "Politics and Knowledge" and is at work on a book to be called "Lessons of the Twentieth Century." He is a graduate of the University of Chicago, B.A. and J.D. In the fall of 1992, his book *Breaking the War System* will be available through Aleithia Press.

Michael Rogin, professor of political science at the University of California, Berkeley, is the author of "*Ronald Reagan*," *the Movie*, and *Other Episodes in Political Demonology* (1987), *Subversive Genealogy: The Politics and Art of Herman Melville* (1983), *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian* (1975), and *The Intellectuals and McCarthy: The Radical Specter* (1967). A graduate of the University of Chicago (Ph.D., 1962), Rogin is currently working on racial cross-dressing and ethnic assimilation in motion pictures.

Robert A. Rosenstone, professor of history at the California Institute of Technology and *AHR* contributing editor for film reviews, is author of, among other books, *Crusade of the Left: The Lincoln Battalion in the Spanish Civil War* (1969) and *Romantic Revolutionary: A Biography of John Reed* (1975). His involvement in the production of several historical motion pictures as adviser and writer led to his current work in the possibilities and problems of the historical film. Rosenstone is also very interested in expanding the formal vocabulary of historical writing. His most recent study, *Mirror in the Shrine: American Encounters in Meiji Japan* (1988), is an experiment in historical narrative, a multi-voiced, self-reflexive work that examines the conditions of its own construction.

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The Paradox of Women's Citizenship in the Early Republic: The Case of *Martin vs. Massachusetts*, 1805

LINDA K. KERBER

No Cit[izen]less to my name, I'll have, says *Katey*, . . . [i]t means, "A *Woman of the town*."

(Boston) *Columbian Centinel*, March 16, 1793.

IN FEBRUARY 1801, JAMES MARTIN submitted a writ of error to the Supreme Judicial Court of Massachusetts, the highest court of appeals in the state. He demanded the return of properties confiscated from his mother twenty years before, toward the close of the American Revolution. In responding to Martin's lawsuit, leading lawyers and judges of the early republic were forced to articulate publicly their understanding—normally left unexpressed—of the nature of women's relationship to the postrevolutionary civic order and particularly their understanding of the obligations married women citizens owed to the state. The outcome was ironic: recognizing the claims of Anna Gordon Martin and her heirs entailed a refusal to construe her as an autonomous citizen with her own civic responsibility.

The political community fashioned by the American war was a deeply gendered one in which all white adults and a few black adults were citizens but white men's voices were privileged. For men, political institutions—the army, the militia, the state legislatures, the Continental Congress, organizations of artisans—facilitated collective experience. A notable male elite, whose patriarchal character has since

This essay was first presented as one of the Jefferson Memorial Lectures, University of California, Berkeley, 1989. I thank the Department of History and the Jefferson Memorial Lectures Committee for many courtesies on that occasion. It was drafted when I was National Endowment for the Humanities Fellow of the Philadelphia Center for Early American Studies (PCEAS) at the University of Pennsylvania in 1987 and at the Center for Advanced Study of the University of Iowa in 1989. I am deeply grateful to the College of Liberal Arts of the University of Iowa for generous support throughout this project and to the John Simon Guggenheim Foundation and the National Humanities Center for support in 1990–1991. This essay could not have been written without the shrewd instruction and guidance I received from Wayne Bodle and other colleagues at the PCEAS, Hendrik Hartog, Bruce Mann, and Alan Taylor. I am grateful for the research assistance of Kathy Hermes of Duke Law School, Leslie Taylor of the University of Iowa, and Alexandra Gillen of the University of Chicago, and for the advice of Sydney V. James, Martha Minow, H. Jefferson Powell, Kathryn T. Preyer, Aviam Soifer, Laurel Thatcher Ulrich, and the anonymous readers for the *AHR*. I received indispensable archival help from Stanley Tozeski, Federal Records Center, Waltham, Mass.; Elizabeth Bouvier, Massachusetts State Archives; and Cynthia Harrison, Federal Judicial History Center, Washington, D.C.

been encoded in its identification as the “Founding Fathers,” articulated political republicanism and embedded it in successive manifestos and institutions. They said they acted in the name of all Americans, even though they formally consulted propertyless white men only rarely and consulted neither black men nor any women, whatever their race or class.¹ Drawing on what they had learned from the great European theorists of the social contract (Hobbes, Locke, Montesquieu, Rousseau), they constituted themselves as citizens whose allegiance was contractual, the corollary of that point being that an allegiance freely chosen could also be annulled.² This aspect of radical ideology sustained yeomen and artisans in making claims on their social betters; it sustained free blacks in making claims on the republic; it sustained those in Congress who devised for the Northwest Territory a pathbreaking ordinance proclaiming that the United States would be an expansive empire without colonialism. Only belatedly, however, would it sustain women in making claims on the republic.

Citizenship involves claims of rights, notably suffrage but also the right to pursue happiness and to be free of constraints. It involves a wide range of civic obligations, among them patriotic loyalty, the payment of taxes, and service on juries and in the military. The political discourse of citizenship has been gendered since its origins, in ways that historians are only beginning to comprehend. “Excluded from honours and from offices, we cannot attach ourselves to the State of Government from having held a place of Eminence,” Abigail Adams observed toward the end of the American Revolution. “Even in freest countrys our property is subject to the controul and disposal of our partners, to whom the Laws have given a sovereign Authority. Deprived of a voice in Legislation, obliged to submit to those Laws which are imposed upon us, is it not sufficient to make us indifferent to the publick Welfare?” Abigail Adams concluded that, given the situation, women’s patriotism must be “the most disinterested of all virtues”; there was practically no way in which women’s service to the state could be directly rewarded. Indeed, one of the few issues on which Patriot and Loyalist men generally agreed was their belief that the only service to the state of which women were capable was financial, and therefore there were few reciprocal obligations—notably, the obligation of single women to pay taxes—that women owed the state.³

Political republicanism in America rested heavily on a definition of citizenship, inherited from the civic republicanism of the Renaissance and of the eighteenth-century British Whig opposition, that favored men for qualities that distinguished them from women. “Luxury, effeminacy and corruption” was as much a revolu-

¹ Further comments on this point are in my essay “‘I Have Don . . . much to Carrey on the Warr’: Women and the Shaping of Republican Ideology after the American Revolution,” in Harriet B. Applewhite and Darline G. Levy, eds., *Women and Politics in the Age of the Democratic Revolution* (Ann Arbor, Mich., 1990), 227–57.

² James H. Kettner, *The Development of American Citizenship, 1608–1870* (Chapel Hill, N.C., 1978), 173 and following.

³ Abigail Adams to John Adams, June 17, 1782, in L. H. Butterfield, et al., eds., *Adams Family Correspondence* (Cambridge, Mass., 1963–), 4: 328. She went on: “Yet all History and every age exhibit Instances of patriotic virtue in the female Sex; which considering our situation equals the most Heroick.” For thoughtful comment on obligation, see Joan R. Gundersen, “Independence, Citizenship and the American Revolution,” *Signs*, 13 (1987): 59–77, esp. 61–62.

tionary-era triad as "life, liberty and the pursuit of happiness."⁴ Republican ideology was antipatriarchal in the sense that it voiced, as Tom Paine accurately sensed, the claim of adult men to be freed from the control of male governors who had defined themselves as rulers and political "fathers" in an antique monarchical system. The language of the revolution was permeated by claims to unsettle traditional patriarchal relationships. The rhetoric of the revolution positioned the colonists as sons who had outgrown patriarchal constraint and as adult men unfairly enslaved by the British.⁵ But republican ideology did not eliminate the political father immediately and completely; rather, it held a liberal ideology of individualism in ambivalent tension with the old ideology of patriarchy. Thus George Washington quickly became the "father of his country"; at the Governor's Palace in Williamsburg, Virginia, the life-size portrait of George III was quickly replaced by a life-size portrait of Washington in the same pose.⁶ The men who remodeled the American polity after the war remodeled it in their own image. Their anxieties for the stability of their construction led them, as they emphasized its reasonableness, its solidity, its link to classical models, also to emphasize its manliness and its freedom from effeminacy. The construction of the autonomous, patriotic, male citizen required that the traditional identification of women with unreliability, unpredictability, and lust be emphasized. Women's weakness became a rhetorical foil for republican manliness.⁷

Yet women were citizens of the nation; they could be naturalized; they were subject to its laws; as single adult women, they were vulnerable to taxation. "The spirit of liberty," in John Adams's famous phrase, "spread where it was not intended." Women's national citizenship contained deep within it an implicit challenge to coverture, the legal system that governed relationships between men and women. Patriot men rarely spoke about this issue, but their actions speak for them. The generation of men who radically transgressed inherited understandings of the relationship between kings and men, fathers and sons, and who radically reconstructed many basic elements of law, nevertheless refused to destabilize the law governing relations between husbands and wives, mothers and children. Coverture—the system of law that transferred a woman's civic identity to her husband at marriage, giving him use and direction of her property throughout the marriage—was incompatible with revolutionary ideology and with the liberal commercial society developing in the early republic. Even as patriot

⁴ See, for example, George Washington to James Warren, October 7, 1785, *Writings of Washington, From the Original Manuscript Sources, 1745–1799*, John C. Fitzpatrick, ed. (Washington, D.C., 1931–44), 28: 290.

⁵ Edwin Burrows and Michael Wallace, "The American Revolution: The Ideology and Psychology of National Liberation," *Perspectives in American History*, 6 (1972): 167–306. See also Winthrop D. Jordan, "Familial Politics: Thomas Paine and the Killing of the King, 1776," *Journal of American History*, 60 (1973): 294–308.

⁶ Now on display in the Museum of Decorative Arts there. Michael Grossberg, *Governing the Hearth: Law and the Family in Nineteenth-Century America* (Chapel Hill, N.C., 1985), also stresses persistence of patriarchal thought.

⁷ Tracing changes in voting law in New Jersey, Gundersen argues that in the early republic, "dependency" was redefined as a "sex-specific trait, [transforming] dependent males into independent voters, while subsuming single women, who were in every practical sense independent, into a category of dependence"; Gundersen, "Independence, Citizenship," 66. See also Ruth Bloch, "The Gendered Meaning of Virtue in Revolutionary America," *Signs*, 13 (1987): 37–58.

men proudly attacked patriarchy, choosing John Locke over Sir Robert Filmer, *Two Treatises over Patriarcha*, they carefully sustained their own patriarchal law.⁸ They even continued to refer to the body of law of coverture by its traditional name, “the law of baron et feme,” that is, not the reciprocal “husband and wife” or “man and woman” but “lord and woman.”⁹

A rare sign that revolutionary legislators glimpsed the incompatibility of coverture with republican ideology is the elimination of the crime of *petit treason* from the statute books of the early republic. The traditional reasoning had been that, at marriage, the husband became the wife’s lord and stood to her as king did to subject; thus the killing of a husband by a wife was analogous to regicide and treason, and the penalties for *petit treason* were worse than those for murder. The killing of a wife by her husband, however, remained murder. The concept was not much enforced in early America, but neither was it eliminated from the statutes until the revolution.¹⁰ With that exception, neither the government of the Articles of Confederation nor the federal government of the Constitution directly challenged the legal system of coverture.

As anthropologists have long understood, the laws and practices of domestic relations echo other aspects of a culture. The American Revolution was preeminently a crisis of authority; a democratizing society and a patriarchal family were discordant. “Do not put such unlimited power into the hands of the Husbands,” Abigail Adams wrote in 1776; “Remember all men would be tyrants if they could.”¹¹ The challenge the revolution posed to patriarchal relationships was expressed dramatically in the excoriation of the father figure of George III in the pages of *Common Sense* and in the ritual pulling down and melting of his statues in the streets. But, as Michael Grossberg has recently emphasized, although the tradition that sustained the authority of the male governor in the home “by granting him control of its inhabitants as well as of family property and other resources” became increasingly anachronistic, it nevertheless persisted in a new order otherwise characterized by “a deep aversion to unaccountable authority and unchecked governmental activism, the equation of property rights with independence, a commitment to self government, a belief that individual virtue could prevent the abuse of power, and a tendency to posit human relations in contractual terms”—all themes that, on the face of it, should have been incompatible with patriarchy.¹² Adult men who were differently situated in terms of

⁸ Carol Pateman, *The Sexual Contract* (Stanford, Calif., 1988), 48–49 *et passim*.

⁹ See, for example, Tapping Reeve, *The Law of Baron and Feme, Parent and Child, Guardian and Ward, Master and Servant, and of the Powers of Courts of Chancery* (1816; 2d edn., Burlington, Vt., 1846). Nathan Dane, *General Abridgment and Digest of American Law* (Boston, 1823), headed the chapter on the relationship between husband and wife “Baron and Feme”; vol. 1, chap. 19, pp. 331–78. William Blackstone, *Blackstone’s Commentaries: With Notes of Reference . . .*, St. George Tucker, ed. (Philadelphia, 1803), vol. 1, book 1, chap. 15 reads, “her husband is called her lord.”

¹⁰ See, for example, “An Act for annulling the Distinction between the crimes of Murder and Petit Treason,” March 16, 1785, in Asahel Stearns, *et al.*, eds., *The General Laws of Massachusetts . . .* (Boston, 1823), 1: 188. *Petit treason* also involved the killing of a master by a servant, and it endured in the slave South longer than in the North. See Kathryn Preyer, “Crime, the Criminal Law and Reform in Post-Revolutionary Virginia,” *Law and History Review*, 1 (1983): 56–58.

¹¹ Abigail Adams to John Adams, March 31, 1776, Butterfield, *Adams Family Correspondence*, 1: 369–70.

¹² Grossberg, *Governing the Hearth*, 5–6.

class—as artisans, farmers, merchants, planters—were similarly situated in regard to gender. Benefiting from the established practice of coverture, they felt no need to renegotiate it. Anxiety combined with self-interest to restrain male legislators from addressing the inherited system.

In stabilizing the revolution and eliminating inherited class distinctions among whites, founding-era legislators, it could be said, created a social system that, while certainly not classless, minimized differences between white men in comparison to what they had been before the war, and they adopted an ideology and a rhetoric that minimized differences between white men even more sharply than these differences were minimized in fact. But the founders kept in place a legal system that promoted legal differences between free women and men, especially between married women and men. They maintained difference while denying its significance.¹³ As men increasingly freed themselves from the constraints of patriarchy, they left in place a fully developed, complex system of law based on the principle that married women were “covered” by their husbands’ legal identity. Even when modest modifications were made in this law (as, for example, on the matter of *petit treason*), legislators of the early republic showed no disposition to reconsider the system as a whole. As both Hendrik Hartog and Carole Shammas have recently observed, the system of law that developed around the *feme covert* was not attacked directly until the second quarter of the nineteenth century and not radically altered until the third quarter.¹⁴ Indeed, major elements have persisted in American law until our own time.

James Otis was one of the few political activists of the revolutionary era who followed the implications of women’s citizenship to their logical end. “If all were reduced to a state of nature,” Otis asked in 1764, not long after his famous Writs of Assistance argument, “had not apple women and orange girls as good a right to give their respectable suffrages for a new King as the philosopher, courtier . . . and politician? Were these and ten millions of other such . . . consulted?”¹⁵ Perhaps in retribution for raising the question at all, he soon went mad.

Were women—even Otis’s apple women and orange girls—part of the new

¹³ “At least until the middle years of the nineteenth century, being married meant subjecting oneself to a known and coercive public relationship,” wrote Hendrik Hartog in an important essay on the conceptualization of marital relations in the early republic. Hartog, “Marital Exits and Marital Expectations in Nineteenth Century America,” *Georgetown Law Journal*, 80 (1991): 95–129, quote 96; see also Hartog, “Mrs. Packard on Dependency,” *Yale Journal of Law and Humanities*, 1 (1988): 79–103. Some years ago, Gerda Lerner warned that women’s status should be evaluated not only in abstract terms but in relationship to changes in men’s status; see Lerner, “The Lady and the Mill Girl: Changes in the Status of Women in the Age of Jackson,” *Midcontinent American Studies*, 10 (1969): 5–15.

¹⁴ “The keystone of family capitalism,” according to Shammas, “was the male household head’s ability to control both his and his wife’s wealth . . . [W]hen women received the legal right during the nineteenth century to possess property that they had inherited, it greatly diminished the percentage of total wealth owned by men. Such legislation, however, did not appear until after corporate forms had evolved sufficiently so that the family was relieved of the chore of being the prime organization involved in the management of capital”; Carole Shammas, “Early American Women and Control over Capital,” in Ronald Hoffman and Peter J. Albert, eds., *Women in the Age of the American Revolution* (Charlottesville, Va., 1989), 154. Shammas discusses this point at greater length in her unpublished essay, “Re-Assessing the Married Women’s Property Acts” (1990), cited by permission of the author.

¹⁵ James Otis, *The Rights of the British Colonies Asserted and Proved* (Boston, 1764), reprinted in Bernard Bailyn, ed., *Pamphlets of the American Revolution 1750–1776* (Cambridge, Mass., 1965), 419–22.

social compact? Or did they remain in a patriarchal social order in which their only freely chosen obligation was to their husbands? As I have argued elsewhere, women who lived through a revolutionary era could not help but explore the potential of the new politics for freeing them to develop their own relationships with the republic. Because the revolutionary generation emerged into the republic with its system of law governing relations between men and women largely intact, those who wished to reconsider the role of women in the republic had little space in which to negotiate. It would be two generations before Elizabeth Cady Stanton and her colleagues could clearly articulate the incompatibility of coverture and liberalism.¹⁶ The old civic republicanism, developed in the Renaissance and transmitted through the British Whig opposition, made property owning and arms bearing central to the political community; in that tradition, women were the timeless representations of ambition, luxury, and lust; qualities understood to be effeminate were qualities to be feared.¹⁷ In this unfriendly theoretical context, critics were able to develop their formal political claims to the compromise point that I have called the politics of "republican motherhood"; that is, they could claim political participation only so long as they implicitly promised to keep their politics in the service of the men of their family, using it to ensure republican authenticity on the part of their husbands and sons.¹⁸

Most historians, including myself, have assumed that this limited definition of female citizenship was all that the revolutionary discourse would permit, that no more fully inclusive definition could be conceived at the time. It is generally thought to be both unfair and ahistorical to expect of the revolutionary generation that it initiate radically new conceptualizations of female citizenship. For the most part, the records on this matter have been silent, and we have tended to take the silence of contemporaries literally, assuming that they were silent about that relationship because they were not thinking about it. But silence itself is a social construction, related to an ability to verbalize and a control of access to the forums of public discussion. It is our task as historians to be alert to occasions on which silence is broken. I now would argue that it *was* possible for contemporaries to conceive of alternatives: they dismantled *petit treason*, and, as we have seen, James Otis opened the broader question. Moreover, it should now be possible for historians to reconsider what counts as citizenship. If we require that discussions of citizenship be limited to claims to voting and office holding by women, then the early republic offers us mostly silence. If we broaden the discussion and recognize that citizenship involves wide-ranging issues of claims of rights and of political behavior, as well as matters of allegiance, support, and analysis, then it is not

¹⁶ Linda K. Kerber, *Women of the Republic: Intellect and Ideology in Revolutionary America* (Chapel Hill, N.C., 1980), chaps. 7 and 9. Carole Pateman, who has written the most shrewd analysis of this point, concludes that coverture meant it was impossible for a wife ever to be the "possessive individual" who is central to the new liberal state, "the proprietor of his person and his attributes," in C. B. Macpherson's terms, the person who, in Locke's words, "has a Property in his own Person. This no body has any right to but himself"; Pateman, *Sexual Contract*, 55–56.

¹⁷ On this point, see Hanna Fenichel Pitkin, *Fortune Is a Woman: Gender and Politics in the Thought of Niccolò Machiavelli* (Berkeley, Calif., 1984).

¹⁸ Linda K. Kerber, "The Republican Mother—Women and the Enlightenment: An American Perspective," *American Quarterly*, 28 (1976): 187–205; "The Republican Ideology of the Revolutionary Generation," *American Quarterly*, 32 (1985): 474–95.

difficult to find many occasions when women addressed political matters. If we adopt an even more inclusive definition, seeking to understand how citizenship is continually being reconstructed, particularly during a postrevolutionary era; if we seek to discern the gendered implications of superficially neutral rhetoric, tracing the ways in which political language was used to emphasize and sometimes redefine the meaning of masculinity; and, if we understand that an ideology that takes enormous pains to exclude women is, by that very fact, an ideology interactive with women, then we can see more clearly how the problem of women's relationship to the state in the postrevolutionary era infused republican ideology and reflected the gender dynamics of the generation that created it.¹⁹

For a moment at the beginning of the nineteenth century, in a Massachusetts courtroom, the discourse opened to suggest that it was not inconceivable that women might be citizens with their own responsibilities to the state. All the participants in this discussion were men; we have no record of a woman's voice in this conversation. Yet, in men's discussion of women, we can hear evidence of the gendered construction of citizenship in the postrevolutionary republic. The case of *Martin vs. Commonwealth of Massachusetts* broke the silence and yielded a counter-revolutionary resolution. It suggests that there had been radical alternatives to the conservative outcome. It suggests how forcefully the courts in one major state were willing to act in order to maintain the gendered relationships on which they understood their community to rest.

JAMES MARTIN'S FAMILY HAD REJECTED a revolutionary connection. His father, William Martin, had been an officer in the Royal Regiment of Artillery, the one regiment in which commissions were not purchased and in which careers were, therefore, open to talent. William Martin was born in England; in 1742, he joined the artillery and was stationed in the colonies during the Seven Years' War. Sometime before 1752, he married Anna Gordon, the daughter of James Gordon, a wealthy Boston merchant and landholder who was briefly a warden of King's Chapel. Over the years, William Martin rose slowly in rank and was posted to a number of places; he and his wife spent some years in Halifax, Nova Scotia. When the revolution broke out, William and Anna Martin fled Boston with the British and traveled to Halifax, then to British-occupied New York City.²⁰

¹⁹ Some reflections on this point appear in Linda K. Kerber, "History Will Do It No Justice: Women and the Reinterpretation of the American Revolution," in Hoffman and Albert, *Women in the Age of the American Revolution*, 3–44. Wayne Bodle explores the implications of one woman's extensive effort to claim compensation from the Royal Commission on American Loyalists in "Jane Bartram's 'Application': Her Struggle for Survival, Stability, and Self-Determination in Revolutionary Pennsylvania," *Pennsylvania Magazine of History and Biography*, 115 (1991): 185–220. See also the important work of Mary P. Ryan, *Women in Public: Between Banners and Ballots 1825–1880* (Baltimore, Md., 1990); and Joan Landes, *Women and the Public Sphere in the Age of the French Revolution* (Ithaca, N.Y., 1988).

²⁰ The Commonwealth of Massachusetts: Middlesex: Court of Common Pleas at Cambridge, December 24, 1782. These Common Pleas records of the revolutionary era have been lost, but sections were reprinted in *Martin v. Commonwealth of Massachusetts*, 1 Mass. Reports, 348 (1805). Other information making it possible to reconstruct William Martin's career appears in Court Files No. 148.112, *Martin v. Gordon*, 1773, Superior Court of Judicature Records, Suffolk County, Archives of the Commonwealth of Massachusetts, Columbia Point, Boston; *Martin v. Gordon*, Record Book,

Ultimately, William Martin served on the staff of Brigadier-General James Pattison; when Pattison returned to England on sick leave in 1780, Martin took his place as commander of artillery until the evacuation of New York in 1783, when he and his family left for England.²¹

What wealth William Martin had seems to have come largely from his marriage. His relationship to that wealth was precarious. After James Gordon's death in 1770, Anna Gordon Martin inherited one-third of his estate—a double share went to her brother. When the property was actually divided in 1773, she received at least 844 acres of land, improved and unimproved, in New Hampshire and central Massachusetts, at least one farm in Braintree, and a house on the Boston Harbor, with a wharf and stables for ten or twelve horses.²²

As was usual in the eighteenth century, even though Anna Martin was a married woman under coverture, real estate that came to her by bequest was set off to her directly, thus keeping it in the Gordon line (assuming that William Martin, who had control of it during his lifetime, did not waste it). Anna Martin had a right of remainder in it, and it would descend to her heirs.²³ William Martin was "seized and possessed" of Anna Martin's properties only "during his natural life."²⁴ William Martin's "ownership" was limited to a life estate in the property;

Superior Court of Judicature, Suffolk County 1773–74, pp. 20–21, June 14, 1773, Archives of the Commonwealth, Columbia Point; "Letters Communicated by Mr. Noble," in Massachusetts Historical Society, *Proceedings*, 2d ser., 13 (1900): 379–96; Massachusetts Archives, ms. vol. 154, p. 253, in Archives of the Commonwealth of Massachusetts, Columbia Point; *A List of the General and Field Officers, As They Rank in the Army* (London, 1763–84); "Memorial of Colonel William Martin of the Royal Regiment of Artillery, Massachusetts & N. Hampshire" (London, 1784), Great Britain, Public Record Office, Records of the American Loyalist Claims Commission, Film 263, Audit Office Transcripts Microfilm [hereafter, AO] 13/47/622–25; "Examination of Colonel William Martin of the Royal Artillery . . . February 5–7, 1789, AO 12/71/102–08. See also AO 13/6/370. Martin is described in E. Alfred Jones, *The Loyalists of Massachusetts: Their Memorials, Petitions, and Claims* (London, 1930), 211; and Peter Wilson Coldham, *American Loyalist Claims* (Washington D.C., 1980), 335.

²¹ Brigadier-General James Pattison to [William Martin], March 31, 1778, Pattison Letterbooks, Microfilm, Royal Artillery Institution Library, Woolwich, England, microfilm copy at the David Library of the American Revolution, Washington's Crossing, Pennsylvania. I am grateful to Wayne Bodle for identifying this letter, as well as other references to Martin in the papers of Guy Carleton, and among the Brigade Orders of the Royal Artillery, Royal Artillery Institution Library, microfilm at the David Library of the American Revolution. See also Public Record Office Microfilm AO 13/6/376. Martin conducted the formal inquiry into the causes of the great fire that destroyed much of New York in 1776; see the "Minutes of a Commission, (1783) to investigate the causes of the Fire in New York City," New-York Historical Society.

²² The house, which was destroyed by the British, sat on a plot of ground that stretched 2,000 feet from the street to low water. It was rented for £30 a year to Major General James Robertson when he was in Boston; AO 12/71/105. See also listing in Massachusetts Historical Society, *Proceedings*, 2d ser., 10 (1895): 180–81. The Braintree property is described in AO 14/47/637–38. The New Hampshire property is described in AO 13/47/622–38. The properties and their disposition can be traced in Suffolk County, Probate Docket vol. 72, pp. 281–84, 524; vol. 76, p. 22; vol. 102, pp. 302, 377, 415; vol. 106, p. 507, and file papers no. 14734, Office of Register of Probate, Suffolk County Courthouse, Boston.

²³ *The Laws Respecting Women, as they regard their natural rights, or their connections and conduct; in which their interests and duties as daughters, wards, heivresses, spinsters, sisters, wives, widows, mothers, legatees, executrizes, &c, are mentioned and enumerated: also, the obligations of parent and child, and the condition of minors . . .* (London, 1777), 183; Dane, *General Abridgment and Digest of American Law*, 341–42; Reeve, *Law of Baron and Feme*, 162.

²⁴ James Martin (Plaintiff in Error) *versus* The Commonwealth and William Bosson and Others, Ter-tenants, *Reports of Cases Argued & Determined in the Supreme Judicial Court* (September 1804–June 1805), 1: 348; hereafter, *Martin v. Commonwealth*. The manuscript file papers related to this case,

he held the property "in right of his wife." In other words, when he died, it would pass not to his heirs but to Anna Martin and her heirs. Because William and Anna Martin fled with the British, they lost all this property; Massachusetts and New Hampshire confiscated it in 1781. Massachusetts seized the property under the Massachusetts Confiscation Act of April 30, 1779, and it was this property for which James Martin sued in 1801.²⁵

Confiscation was one among many ways in which patriots defined theirs as an authentically new social order. Virtually every state gave force to this commitment with a fairly standard trio of statutes, solidly in place by 1778, linking treason, oaths of allegiance, and the establishment of a system of confiscation. Together, the three were gestures of commitment to the revolution; the penalties could not be easily undone or forgiven. Treason—the act of aiding the enemy—and misprision of treason—the knowledge and concealment of an enemy plot—was to be punished with death, banishment, or enforced service on a naval vessel (from which escape was virtually impossible). Oaths, in the eighteenth-century religious system, were understood to be existentially binding. And the confiscation of property reached not only to self-interest but to the conditions for individual autonomy. The state of Massachusetts was unusual in the care it took to include women in each aspect of the triad.

Throughout the nation, treason and misprision of treason were understood to be crimes that either sex might commit. The statutes were written in terms of "persons." The penalty provisions sometimes eschewed the generic male pronoun in favor of the specific: North Carolina provided, for example, that those judged guilty of high treason "shall suffer Death without Benefit of Clergy, and his or her Estate shall be forfeited to the State."²⁶ The New Jersey statute was similar, but it

including the writs submitted to the court and detailed descriptions of the property involved, are in the records of the Supreme Judicial Court, Suffolk County, March 1805, Archives of the Commonwealth of Massachusetts, Columbia Point, Boston.

²⁵ "An Act to Prevent the Return to this State of Persons therein named . . . who have left this State . . . & joined the Enemies thereof," October 16, 1778, in *The Acts and Resolves, Public and Private, of the Province of Massachusetts Bay* . . . (Boston, 1886), vol. 5, chap. 24, pp. 912–18. William Martin was named in this statute. Massachusetts Archives (manuscript), vol. 154, p. 338; October 3, 1781, account of sale of estates of "Conspirators and Absentees," Archives of the Commonwealth of Massachusetts, State House, Boston. The only closely parallel situation I have been able to find is a suit apparently brought in the U.S. Circuit Court by Ward Nicholas Boylston to recover his family estate in Roxbury. It had been confiscated from his father, Benjamin Hallowell, who had been named as a "notorious conspirator" in the statute of April 13, 1779; Boylston (who had taken his mother's maiden name) claimed, as James Martin would do, that the property had actually been owned by his mother, and consequently the confiscation could operate "upon the life estate of Mr. Hallowell, in right of his wife, but did not convey the fee." The estate had been acquired by Issac Rand, who sold it to a French immigrant, Dr. Lewis Leprilete, in 1791. Boylston's suit was successful in 1803; that success might have prompted James Martin's action. George Blake, who had served as Leprilete's lawyer, would make an opposite argument as Martin's lawyer. See "Confiscated Estates," *New England Genealogical and Historical Register*, 12 (1858), 71–72; Jones, *Loyalists of Massachusetts*, 281–84. I am deeply grateful to David Maas for this reference. *Ward Nicholas Boylston v. Lewis Leprilete*, U.S. Circuit Court, Mass. District, October 1801, copy in Court of Common Pleas, Suffolk County, files, January 1802, *Leprilete v. Rand*, Judicial Archives, Archives of the Commonwealth, Columbia Point, Boston. See also Supreme Judicial Court, Suffolk County, Record Book, 1803–1804, *Rand v. Leprilete*, Judicial Archives, Archives of the Commonwealth.

²⁶ April 1777, in *First Laws of the State of North Carolina* (Raleigh, N.C., 1791), 1: 284. A New York statute against spying provided that "he or she" might be put to death after courtmartial. See *Laws of New York*, vol. 1, June 30, 1780, p. 282; and March 30, 1781, p. 370. See also discussion in Bradley

added the provision that the condemned person might receive a pardon if he enlisted in the Continental Navy. Since women were not welcome in the navy, an alternative punishment was provided for them: a fine of up to three hundred pounds and imprisonment for up to one year.²⁷

Massachusetts' 1777 treason statute made an even more explicit claim to women's allegiance. The statute applied to all "Members" of the state, who were defined as "all persons abiding within . . . and deriving protection from the laws." When the General Court defined the punishments for treason, it made sure that the implication of "all persons" was clear: "every person who shall be attainted of treason within this State, whether male or female, shall be punished by being hanged by the neck until they are dead."²⁸ Although the statutes explicitly applied to women, and women were accused of misprision of treason, it was exceedingly rare for a woman to be accused of treason. No person was executed for treason in Massachusetts in the course of the revolution.²⁹

The gendered dimension of oaths of allegiance was more murky. Although occasionally framed in terms of all residents, oaths seem almost always to have been selectively imposed on men. The strategy of requiring oaths assumed a community of Christians who truly believed their immortal souls to be at risk should they forswear themselves. When tension built between the colonies and the empire in the 1770s, tests of loyalty were instituted. Throughout the colonies, women were encouraged to support boycotts by changed patterns of consumption. But only in Massachusetts were women also explicitly included among the signatories of the evidence of commitment, the Solemn League and Covenant, drafted by the Assembly and circulated in the early summer of 1774. Contemporaries report "every adult of both sexes putting their names to it, saving a very few."³⁰

Those who refused to swear loyalty when it was demanded of them faced physical and financial punishment. Even the most primitive government provides the basic service of overseeing the orderly transmission of property—by exchange, sale, or inheritance. This was a service the patriots were absolutely committed to maintain, for their understanding of a new political order rested

Chapin, *The American Law of Treason: Revolutionary and Early National Origins* (Seattle, Wash., 1964), 37–45; and my discussion in *Women of the Republic*, 121–23.

²⁷ "Act for Constituting a Council of Safety," September 1777, in *Acts of General Assembly of the State of New Jersey . . .* (Trenton, N.J., 1784), 87.

²⁸ "An Act Against Treason and Misprision of Treason," passed by the General Court, February 1, 1777, when sitting in exile from Boston at Watertown. *Acts and Resolves, Public and Private, of the Province of Massachusetts Bay* (1886), vol. 5, chap. 32, p. 615. The Massachusetts statute was closely modeled on the treason statute of the Continental Congress, June 24, 1776: "That all persons residing within any of the United Colonies, land deriving protection from the laws of the same, owe allegiance to the said laws, and are members of such colony"; *Journals of the Continental Congress*, 5: 475–761; quoted in Kettner, *Development of American Citizenship*, 179. For an insightful examination of the gradual transformation of the concept of the king's "subject" to the republic's "citizen," see Kettner, chap. 7.

²⁹ Chapin, *American Law of Treason*, 46. There were no executions in Virginia, either. There were dozens of arrests for treason in New York, New Jersey, and Pennsylvania, however, and some executions. Chapin, 48–62.

³⁰ Harold M. Hyman, *To Try Men's Souls: Loyalty Tests in American History* (Berkeley, Calif., 1960), 63–64.

solidly on the guarantee of the security of property. Moreover, patriots' understanding of individual independence and autonomy assumed that the active citizen had sufficient property to assure that he acknowledged no one as master. But patriots felt no obligation to oversee the orderly transfer of *loyalist* property down through a series of heirs. Including the explicit refusal to do so was one of the ways in which patriots defined the Rubicon they had crossed.³¹

To confiscate a loyalist's property required the property to be identified precisely. In English law, coverture ensured that the property a married man held was almost never fully his, free and clear; one-third of his real estate was encumbered to his wife for her use as dower property during her widowhood—that is, until she remarried or died. This dower property (the widow's thirds) was understood to be an equitable recompense for the woman who had given up control of her property at marriage, and British law treated her rights to it with great care. The dower property even of widows of men who had been hanged, drawn, and quartered for treason was carefully preserved. In confiscating loyalist property, most patriot legislatures left dower as a recognized claim on the estate. They did so partly because of long-established tradition, partly on the practical grounds that to assign dower would prevent the wives and children of absentees from becoming a burden on public charity (although their standard of living would obviously drop precipitously). This restraint was congruent with the generally restrained approach American revolutionaries took toward the law of inheritance.³²

Confiscation statutes varied widely in their treatment of dower right; most left the matter to implication. Connecticut and New York made no mention of dower right at all.³³ Rhode Island and South Carolina not only made no mention of

³¹ The fullest account of confiscation in revolutionary Massachusetts is found in David E. Maas, "The Return of the Massachusetts Loyalists" (Ph.D. dissertation, University of Wisconsin, 1972), esp. chap. 6. Maas stresses the complications and contradictions of Massachusetts policy: the fears of patriots when the British occupied Boston, the self-interest of private citizens who helped themselves to the property of absentees, the desire not to leave abandoned members of loyalist families dependent on patriot public charity—"leaving a wife in Massachusetts was the major method of protecting real estate"; p. 314. After the war, Thomas Astney, the British agent for loyalist claims, was surprised at how little the rebellious state government had profited from confiscation; p. 270. Maas concludes that, in Massachusetts, confiscation "failed as a method of financing the revolution"; p. 313. This failure made it practical for Tories to return and reclaim their land. Maas replaces the older, and much briefer, review of confiscation legislation in Massachusetts: Andrew McFarland Davis, "The Confiscation Laws of Massachusetts," *Publications of the Colonial Society of Massachusetts*, 8 (1903): 50–72. Although Harry Yoshpe does not deal directly with the extent to which confiscation served as "a method of financing the revolution," his conclusions for New York are roughly congruent with those reached by Maas; see Harry B. Yoshpe, *The Disposition of Loyalist Estates in the Southern District of the State of New York* (New York, 1939).

³² See Stanley N. Katz, "Republicanism and the Law of the Inheritance in the American Revolutionary Era," *Michigan Law Review*, 76 (1977): 28. Carole Shammas, Marylynn Salmon, and Michel Dahlin, *Inheritance in America from Colonial Times to the Present* (New Brunswick, N.J., 1987), also stresses the limits of the changes made in inheritance law in the revolutionary era, see esp. 63–79.

³³ "An Act directing certain confiscated Estates to be sold," *Acts and Laws of the State of Connecticut in America* (New London, Conn., 1794), 56–57. "An Act, for the forfeiture and sale of the estates of persons who have adhered to the enemies of this State . . . 22 October 1779," *Laws of the State of New York* . . . (Albany, N.Y., 1886), 1: 173–84. See also "An Act for the speedy sale of the confiscated and forfeited estates . . . 12 May 1784," *ibid.*, 736–59. But after the revolution, the New York legislature passed a number of private bills that restored dower claims. See, for example, "An Act for the relief of Clara Service and others," 3: 717 and following.

dower but also claimed all property of loyalist absentees, whether held "in possession, reversion or remainder."³⁴ Others, among them New Jersey, New York, and Delaware, made no mention of dower right but may have included it by implication in the "just debts" to be liquidated before confiscated property was transferred or sold.³⁵ A few states, New Hampshire and Pennsylvania, did not mention dower right directly but did provide for the support of the absentees' families out of confiscated estates.³⁶ In Georgia, a Board of Commissioners in each county, charged with confiscation and sale, could allocate half the estate—and if the estate was very small, all of it—to a wife or child left behind.³⁷ Only a few states, however, Virginia, North Carolina, and Massachusetts, specified that dower would be recognized if the woman had stayed in America.³⁸

³⁴ The quoted words are from "An Act for disposing of certain estates," February 26, 1782, in John Faucheraud Grimke, ed., *The Public Laws of the State of South Carolina . . .* (Philadelphia, 1790), 204 and following. The language of the Rhode Island statute covered "all and every the lands, tenements and hereditaments whatsoever, whether held in fee simple, fee tail, for term of life or years; and all estates in remainder or reversion, and all goods and chattels, rights and credits of every kind"; "An Act for the confiscating the estates of certain persons . . ." (October 1779), in John Russell Bartlett, ed., *Records of the State of Rhode Island and Providence Plantations in New England* (Providence, R.I., 1863), 8: 609–14. That the committees appointed to supervise the sales of these properties did not automatically reserve the dower right is suggested by the fact that Sarah Wanton had to petition for support for herself and son from the rents of a portion of her deceased husband's confiscated estate. March 1781, *Records of the State of Rhode Island*, 9: 350–51.

³⁵ "An Act for taking Charge of and leasing the Real Estates, and for forfeiting the Personal estates of certain Fugitives and Offenders . . ." April 18, 1778, *Acts of the General Assembly of the State of New Jersey*, 43–52; "An Act for forfeiting to, and vesting in, the State of New Jersey, the Real Estates of certain Fugitives . . ." December 11, 1778, *ibid.*, 67–68; "An Act to direct the Agents of forfeited Estates . . . to proceed to the Sale . . ." December 16, 1783, *ibid.*, 354–56; "An Act of free pardon and oblivion . . ." June 26, 1778, *Laws of the State of Delaware* (New Castle, Del., 1797), 2: 636–43; "A Supplement to an act, intituled, An act of free pardon . . ." June 5, 1779, *ibid.*, 658–65; "An Act, for the forfeiture and sale of the estates of persons who have adhered to the enemies of this State . . ." October 22, 1779, *Laws of the State of New York [1777–1784]* (Albany, N.Y., 1886), 1: 173–84; "An Act for the speedy sale of the confiscated and forfeited estates . . ." May 12, 1784, *ibid.*, 736–59.

³⁶ In Pennsylvania, the justices of the Supreme Court could set aside parts of forfeited estates to support wives and children of absentees and traitors; dower may have been treated as an encumbrance on the estate. See "An act for the attainder of divers traitors . . . 6 March 1778," *Laws of the Commonwealth of Pennsylvania* (Philadelphia, 1803), 2: 165 and following, esp. 173–76. New Hampshire provided that committees of confiscation (in 1778) and probate judges (in 1779) could "make such allowance for the wives and children of such absentees out of their estates as they shall judge proper." No specific mention was made of dower or of the necessity to reside in the United States. "An Act in Addition to an Act intituled 'An Act to Confiscate the Estates of Sundry Persons' . . . 26 June 1779," in Henry Harrison Metcalf, ed., *Laws of New Hampshire . . .* (Bristol, N.H., 1916), 4: 216–18.

³⁷ "An Act for attainting such persons as are therein mentioned of high treason, and for confiscating their estates . . . 1778," in Robert and George Watkins, eds., *A Digest of the Laws of the State of Georgia . . .* (Philadelphia, 1800), 208 and following, esp. sect. 22: 218. See also "An Act for inflicting penalties on, and confiscating the estates of such persons as are therein declared guilty of treason . . ." 1782, *ibid.*, 242 and following, sect. 12.

³⁸ In Virginia, widows or wives without children could claim dower, and the property of British subjects with children, whether the wife was alive or not, was exempt from confiscation if the heirs resided in the state. William Waller Hening, ed., *The Statutes at Large: Being a Collection of All the Laws of Virginia . . .* (Richmond, Va., 1823), 10: 71, chap. 14, May 1779. For a discussion of loyalist wives who remained, see Adele Hast, *Loyalism in Revolutionary Virginia: The Norfolk Area and the Eastern Shore* (Ann Arbor, Mich., 1982), 127–31. North Carolina also reserved dower claims for wives of absentees who remained "in or under the protection of the . . . United States." They were "allowed" as much of the confiscated estate as if the absentee had died intestate, that is, their dower right, which in North Carolina was one-third of the lands and a portion of the personal property. North Carolina added the provision that in small estates, wives of absentees could receive "so much of the personal property, including all the household goods . . . as will be sufficient for the reasonable support of the wives,

The Massachusetts legislation was "ardent," wrote one of its historians, and did not leave dower claims to implication.³⁹ The statute that passed in 1779, after several drafts, provided "That where the wife, or widow, of any of the [loyalists] aforementioned and described, shall have remained within the jurisdiction of any of the said United States, and in parts under the actual authority thereof, she shall be [e]ntitled to the improvement and income of one third part of her husband's real and personal estate (after payment of debts) during her life, and continuance within the Said United States; and her dower therein shall be set off to her by the judges of probate of wills, in like manner as it might have been if her husband had died intestate and a liege subject of this state."⁴⁰ The explicit requirement that she remain in the state implied that wives of absentees who fled with their husbands could not claim dower in confiscated property. An early draft was even stronger; its explicitly revolutionary preface attacked the king, it required that debts owed to Americans were the first lien on the estates and that proceeds from the sale of loyalist estates be used to relieve both inhabitants who had suffered in the war and the wives and children of dead American soldiers.⁴¹ There is no parallel to this last clause in the confiscation legislation of any other state.

By spelling out what was probably the usual expectation throughout the United States, Massachusetts made that expectation into a self-limiting proposition: not that most cases would relate to wives who stayed on but that all cases should. Thus Massachusetts enacted a statute that contained within it a deeply radical and broader set of claims for a revolutionary relationship between the married woman and the state. Anna Martin's heir did not claim her dower property; he claimed property that she had held in her own right. But because she had fled the

widows and children," and if the lands were small and impoverished, the county court might set aside all of it for the support of wife and children. "An act to Carry into effect an Act . . . entitled An Act for Confiscating the Property . . . 1778," in Walter Clark, ed., *The State Records of North Carolina* . . . (Goldsboro, N.C., 1905), vol. 24, Laws 1777–1788, pp. 209–14. Note esp. sects. 6, 15. North Carolina went further than other states by specifying that confiscated properties could also be reserved for the support of aged parents of the absentee who were otherwise unable to maintain themselves. See "An Act to carry into effect . . . Act for confiscating the property of all such persons as are inimical [of November 1777]," Clark, vol. 24, p. 268, sect. 17; and "An Act directing the sale of Confiscated Property," Clark, pp. 424–29 (1782), chap. 6, sect. 21.

³⁹ Richard D. Brown, "The Confiscation and Disposition of Loyalists' Estates in Suffolk County, Massachusetts," *William and Mary Quarterly*, 3d ser., 21 (1964): 537. Brown distinguishes between the legislation, which he characterizes "as the most ardent and vengeful . . . justifying the confiscations in blatant Whig terms," and the modest extent of its enforcement: "Except for a few conspirators, none of the loyalists was ruined by confiscations carried out for purely, or even principally, political reasons . . . The General Court's most active concern, both in sequestration and confiscation, was always the protection of creditors"; p. 550.

⁴⁰ "An Act for Confiscating the Estates of Certain Persons Commonly Called Absentees," May 1, 1779, chap. 49, *Acts and Resolves, Public and Private, of the Province of Massachusetts Bay*, 5: 968–69. Details of the legislative history of this statute appear on 1053 and following. On the law of dower, see Marylynn Salmon, *Women and the Law of Property in Early America* (Chapel Hill, N.C., 1986), chap. 7. The efforts of one absentee's wife to have set off to her one-third of her husband's real estate, "for her dower therein during her life & continuance within the United States of America" may be traced in detail in the John Chandler Transcripts, Octavo Volume C and Folio Volume C, American Antiquarian Society, Worcester, Massachusetts. See also Andrew McFarland Davis, *The Confiscation of John Chandler's Estate* (Boston, 1903).

⁴¹ Manuscript draft, October 8, 1778. Acts 1778, chap. 48, Archives of the Commonwealth of Massachusetts, Columbia Point, Boston. This packet includes three other versions drafted between October 1778 and April 1779.

revolution, the state of Massachusetts did not think the property should be returned to her estate.

JAMES MARTIN WAS FORTY-EIGHT YEARS OLD, and the twenty-year statute of limitations was about to run out when he brought his complaint to the Supreme Judicial Court.⁴² His case was heard by a four-judge panel—Francis Dana, Theodore Sedgwick, Simeon Strong, and George Thatcher—all men who were Federalist by conviction and deeply conservative by temperament. The careers of the judges had involved decades of extensive interaction with each other and with the attorneys who now argued both sides of the *Martin* case.⁴³ The political affiliations of the opposing pairs of attorneys—there was a Democratic-Republican and a Federalist on each team—suggests how close, even overlapping, political positions could be in the early republic, especially among members of the political and economic elite.⁴⁴

Martin's case was presented by George Blake, a well-established Boston lawyer who had been appointed to the "lucrative" office of federal district attorney by the Jeffersonians in 1801.⁴⁵ Blake situated himself on the conservative edge of the Republican Party; it was often difficult to tell him from a Federalist. Some years later, for example, Blake became a leading opponent of the effort to achieve full disestablishment of religion in Massachusetts; he allied himself with conservative Federalists on the issue and fought vigorously for nearly two decades. In the 1820s and 1830s, when moderate Jeffersonians had moved into the ranks of Jacksonian Democrats, Blake was a leader of the Whigs, along with conservative Jeffersonians and many former Federalists.⁴⁶

Blake's colleague in Martin's claim was Theophilus Parsons, a prominent

⁴² *Martin v. Commonwealth*, 348 and following.

⁴³ On the longstanding tension between Dana, Sullivan, and Sedgwick, see Thomas Dwight to Theodore Sedgwick, March 20, and April 21, 1798, Theodore Sedgwick Papers, Vol. C, Massachusetts Historical Society; between Parsons, Dana, and Sullivan, see Theophilus Parsons II, *Memoir of Theophilus Parsons* (Boston, 1859), 79; for tension between Parsons and Sedgwick, see Richard E. Welch, Jr., "The Parsons-Sedgwick Feud and the Reform of the Massachusetts Judiciary," *Essex Institute Historical Collections*, 92 (1956): 171–87. For Sullivan's role in shaping the postwar judiciary system in Massachusetts, see "Bills Drawn by James Sullivan, 1782," in Robert Treat Paine Papers, Massachusetts Historical Society. On the culture of courts and lawyers in this period, see Gerard W. Gawalt, *The Promise of Power: The Emergence of the Legal Profession in Massachusetts 1760–1840* (Westport, Conn., 1979), chaps. 2–3; and Van Beck Hall, *Politics without Parties: Massachusetts 1780–1791* (Pittsburgh, 1972), 46–48.

⁴⁴ Oscar Handlin and Mary Flug Handlin pointed this out some years ago, but they made a distinction between national and local issues that now seems arbitrary. See Handlin and Handlin, *Commonwealth: A Study of the Role of the Government in the American Economy, Massachusetts, 1774–1861* (1947; Cambridge, Mass., 1969), 57–58.

⁴⁵ Thomas C. Amory, *Life of James Sullivan: With Selections from His Writings* (Boston, 1859), 2: 88. George Blake (1769–1841) graduated from Harvard in 1789 and read law with James Sullivan; he was admitted to the bar in 1792 and served as U.S. attorney 1801–1809. Alfred S. Konefsky and Andrew King, eds., *The Papers of Daniel Webster: Legal Papers*, Vol. 1: *The New Hampshire Practice* (Hanover, N.H., 1982), 137.

⁴⁶ Blake's positions on disestablishment can be traced in William G. McLoughlin, *New England Dissent 1630–1883: The Baptists and the Separation of Church and State* (Cambridge, Mass., 1971), 2: 1074, 1158, 1173, 1177–78, 1218–19. His later legal career and his friendship with Daniel Webster can be followed in Charles M. Wiltse, ed., *The Papers of Daniel Webster: Correspondence* (Hanover, N.H., 1974, 1976), vols. 1 and 2.

Federalist with whom John Quincy Adams had studied law. The following year, Parsons was elevated to the post of chief justice in the court in which he now appeared as attorney. Parsons had close Tory connections and was a leader of the most conservative wing of the Massachusetts Federalists, among whom Tory sympathies were common.⁴⁷

The state's lawyers also came from both parties. Daniel Davis, the solicitor general for the Commonwealth of Massachusetts, had built his career on the Maine frontier. He was a moderate Federalist, whom Jefferson promptly removed as U.S. attorney for Maine in 1801; Massachusetts' Federalist governor, Caleb Strong, had equally promptly appointed Davis solicitor general. Davis held the office for the rest of his professional life. Although he wrote two minor treatises, he was known primarily as a solid trial lawyer; a memoirist conceded "he was not a student, nor a book lawyer, but was quick in his perceptions, and argued his cases well."⁴⁸ Davis's colleague was the state's attorney general, James Sullivan, who at the time of the trial was the Republican candidate for the governorship, challenging Caleb Strong's bid for reelection.⁴⁹

James Sullivan's historical reputation has been eclipsed by that of his more famous brother John, a revolutionary war general. But James Sullivan played an active and sometimes ambiguous role in Massachusetts politics throughout the early republic. If we are to understand the context in which he and Davis made their arguments against Martin's claim, it is important to understand his career, in all its complexity and occasional contradiction. Sullivan was a Jeffersonian who sometimes played the Federalists' game, sometimes resisted it; Ronald Formisano locates him as one of the "Republicans of the revolutionary center."⁵⁰

⁴⁷ For Parsons' politics, see Parsons, *Memoir of Theophilus Parsons*, 27 and following; and David Hackett Fischer, *The Revolution of American Conservatism* (New York, 1965), 254. Fischer stresses Parsons' elitism: about the Massachusetts constitutional convention of 1779–1780, Fischer writes, Parsons "worked at cross-purposes with John Adams . . . [and sought to extend the power of the 'natural aristocracy'] throughout the government, without check or balance." For Parsons' conservative views of marriage, see Grossberg, *Governing the Hearth*, 72–73. James Sullivan's biographer stresses the collegial relations the two men maintained on the circuit; see Amory, *Life of James Sullivan*, 2: 39–42. Daniel Webster was then a young man reading law in Boston; his admiring comments on Parsons and his critical comments on Sullivan appear in his note "Some Characters at the Boston Bar, 1804," in Konefsky and King, *Papers of Daniel Webster*, 1: 41–42.

⁴⁸ William Willis, *A History of the Law, the Courts and the Lawyers of Maine from Its First Colonization to the Early Part of the Present Century* (Portland, Me., 1863), 114–15. Willis says that the office of solicitor general had been "created especially" for Davis, and when he retired from it, the office was eliminated. I am grateful to Alan Taylor for this reference.

⁴⁹ See (Boston) *Columbian Centinel*, March 6, 13, April 20, 1805. Not until 1806 did a Boston newspaper mention the Martin case or Sullivan's connection with it. Then William Bosson, who had purchased the confiscated property and lost it in the lawsuit, charged that Sullivan, though acting on behalf of the state as attorney general, had also and inappropriately charged him (Bosson) a twenty-dollar fee for legal services. Sullivan defended himself in print; see *Boston Chronicle*, March 24 [?], 1806, reprinted in Amory, *Life of James Sullivan*, 2: 413–14. On the election rivalry, see Moses Sampson, *Who Shall be Governor, Strong or Sullivan? Or, the Sham-Patriot Unmasked* (Hudson, N.Y., 1806). For Sullivan's bitter reflections after the election, see Sullivan to Thomas Jefferson, June 20, 1806: "I am now before a Supreme Judicial Court, artful, malignant, and cruel, fully determined to ruin me if possible. The Governor [Caleb Strong] will give them all the countenance he can . . . If I would retire to private life, they would not think me worth their pursuit [*sic*], but I can not endure the idea that these malignant men should review me as in a situation chased away by the federalists, and neglected by my own party." See also Sullivan to Jefferson, June 20, 1805, April 21, 1806, Sullivan Papers, Massachusetts Historical Society.

⁵⁰ Ronald Formisano, *The Transformation of Political Culture: Massachusetts Parties, 1790–1840* (New

Born in Berwick, Maine, in 1744, son of an Irish immigrant, Sullivan was sixty-one years old at the time of the *Martin* trial. Although he lacked, in John Adams's words, "an Accademical education," had begun "with neither learning, books, Estate, or any Thing, but his Head and Hands," suffered from epilepsy, and was badly lame as a result of a boyhood accident, Sullivan moved quickly from the frontier to the town of Durham to study law with his equally ambitious older brother John, and married Hetty Odiorne, daughter of a shipbuilder and royal commissioner.⁵¹ When the Boston Port Bill closed the city's trade, Sullivan was elected to the General Court from Biddeford; thereafter, he remained in public service, on one revolutionary committee or another, in the General Court, the Provincial Congress, or as attorney general.⁵² He was a member of the General Court while the statutes regulating the return of absentees and the confiscation of their property were debated and enacted.⁵³ During his time in the Provincial Congress, Sullivan endorsed strong, even punitive confiscation law.⁵⁴ After the war, Sullivan was often found on the side of those who were making it difficult for Tories to return.⁵⁵

Sullivan's strong support for the revolution was buttressed by his admiration for his heroic brother, General John Sullivan, and by his grief for another brother, Daniel, who died on a British prison ship. Sullivan had made his career in the crucible of the revolution. He understood the society of the early republic to be a postrevolutionary one, committed to the promises the revolutionary generation had made. He was hesitant when the Federal Convention bypassed the

York, 1983), 60 and following. Sullivan exchanged more letters with Jefferson than have been published; those published show Jefferson urging Sullivan to be of good cheer despite Federalist attacks. See Jefferson to Sullivan, February 9, 1797, in Paul Leicester Ford, ed., *The Writings of Thomas Jefferson* (New York, 1896), 7: 116–18; Jefferson to Sullivan, May 21, 1805, 8: 354–56, and Jefferson to Sullivan, June 19, 1807, 9: 75–78, congratulating him on his election as governor of Massachusetts.

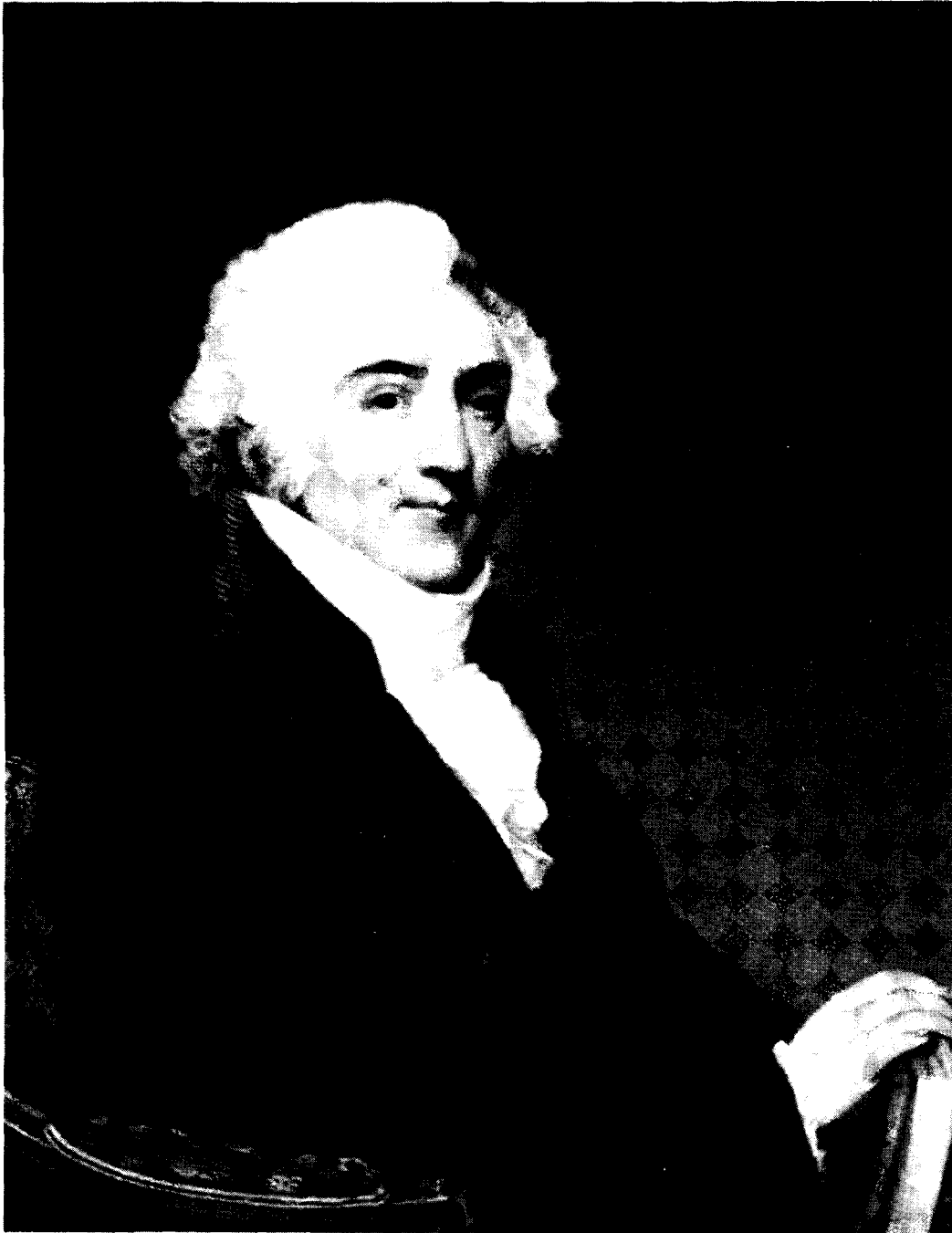
⁵¹ John Adams to Abigail Adams, June 29, 1779, *Adams Family Correspondence*, 1: 113. Amory, *Life of James Sullivan*, 1: 28. The best biographical essay on Sullivan is Clifford K. Shipton's; see *Sibley's Harvard Graduates: Biographical Sketches of Those Who Attended Harvard College . . .* (Boston, 1970), 15: 299–322. From the pre-revolutionary war years and throughout their lives, Sullivan supported Sam Adams and John Hancock. This often meant that he was to be found in alliances that John Adams opposed; indeed, in 1788, Sullivan tried to gather support for Hancock instead of Adams as vice-president. For Sullivan's defense of Stamp Act rioters and Adams's defense of their target, see *Adams Family Correspondence*, 1: 132–33.

⁵² Biddeford, Maine, elected Sullivan to its Committee of Safety and sent him as its delegate to three pre-revolutionary congresses between 1774 and 1775. In 1776, he became its delegate to the new state legislature. In 1778, Sullivan moved to the central Massachusetts town of Groton and soon afterward was appointed to the Superior Court of Judicature, the predecessor of the Supreme Judicial Court. He continued to serve concurrently in the legislature; he later moved to Boston.

⁵³ Samuel A. Green, *An Account of the Lawyers of Groton, Massachusetts, Including Natives Who Have Practised Elsewhere, and Those Also Who Have Studied Law in the Town* (Groton, Mass., 1892), 22 and following.

⁵⁴ James Sullivan to James Warren, March 24, 1777, in Amory, *Life of James Sullivan*, 1: 96, see also 64.

⁵⁵ Amory, *Life of James Sullivan*, 1: 146–48. Sullivan served on legislative committees that drafted stiff laws for returning Tories in 1784; these drafts were ultimately modified because they were not congruent with the promises of the Treaty of 1783. See Maas, "Return of the Massachusetts Loyalists," 450–51, 475; and James Sullivan to Richard Henry Lee, April 11, 1789, James Sullivan Papers, Massachusetts Historical Society, cited in Maas, 504. Sullivan was a lawyer for those who resisted payment of debts to Tories (despite the provisions of the peace treaty); Maas, 528, see also 447 n. 8, 475, and 508 n. 27.



James Sullivan, in an 1807 engraving by H. Wright Smith, after a painting by Gilbert Stuart. Courtesy of the Massachusetts Historical Society, Boston.

amendment procedures of the Articles of Confederation, was skeptical of the Three-Fifths Compromise, and worried about the future of civil liberties under the new Federal Constitution in view of the absence of a bill of rights with a strong

guarantee of trial by jury.⁵⁶ When the French Revolution broke out, Sullivan wrote at least one pamphlet supporting it and emphasizing its parallels with the American experience.⁵⁷ While he was arguing the *Martin* case, Boston's major Federalist newspaper castigated Sullivan as a "Jacobin" and alleged that, nine years before, Sullivan had been part of a "self-created society" whose politics echoed those of "the mother club, in Paris, under Robespierre and Marat."⁵⁸

Nevertheless, in the politics of Maine, and in Massachusetts politics in general, Sullivan played a cautious role. He looked to metropolitan Boston for his alliances, not to the explosive backcountry. If he attacked some returning Tories, there were others whom he assisted.⁵⁹ He was unambivalently opposed to Daniel Shays and his rebellion. As attorney general, Sullivan aggressively prosecuted the state's interest against the proprietors of large speculative endeavors, but he had also, Alan Taylor reports, "been a lawyer for both the Plymouth Company and for Henry Knox," one of the largest land speculators in Maine.⁶⁰ Sullivan's fullest statement of his understanding of the marketplace is his 1792 pamphlet, *The Path to Riches*, in which he bitterly denounced Alexander Hamilton's Bank of the United States for favoring speculators in stocks and bonds and strongly urged making bank stock available to small investors.⁶¹ Although he had grown wealthy not only as a result of shrewd land purchases but also because of the elite connections of his father-in-law, Sullivan understood himself to be a self-made man, devoted to the free market, certain that in earning his wealth he had done nothing but what the free play of the market permitted. He was, in short, a moderate Jeffersonian and a classic liberal.

Sullivan was consistent in his liberalism, committed to making "all law compat-

⁵⁶ Amory, *Life of James Sullivan*, 1: 221. See also [James Sullivan], *A Dissertation upon the Constitutional Freedom of the Press . . .* (Boston, 1801).

⁵⁷ In this pamphlet, Sullivan powerfully defined the United States as a postrevolutionary society. If Americans were unrepentant—as he thought they should be—for tar and feathers, for the humiliation and jailing of traitors, for confiscated estates, then they had ought not castigate the French for similar vigor; indeed, they ought to acknowledge their similarities and their debt. Novion [James Sullivan], *The Altar of Baal Thrown Down: Or, the French Nation Defended, Against the Pulpit Slander of David Osgood, A.M. Pastor of the Church in Medford* (Boston, 1795). *An Impartial Review of the Causes and Principles of the French Revolution, by an American* (Boston, 1798) is also attributed to Sullivan; in this, too, he makes comparisons between the French and American experience; see esp. 33, 65, 84, 98.

⁵⁸ *Columbian Centinel*, March 23, 1805. Sullivan denied that he had written the group's constitution but insisted there was nothing wrong with the Massachusetts Constitutional Society. (Boston) *Independent Chronicle*, March 11, 1805.

⁵⁹ Amory, *Life of James Sullivan*, 1: 138 and following. Sullivan undertook the defense of the returning Tory Sylvester Gardiner in return for title to one of Gardiner's estates, an arrangement that was revealed only by David Maas in 1972 (Maas, "Return of the Massachusetts Loyalists," 526). Another Tory he assisted was William Vassall (Maas, 533). By 1787, he was in collegial correspondence with Richard King's son, the Federalist senator Rufus King.

⁶⁰ In 1798, Judge Francis Dana nearly threw Sullivan out of the courtroom for his expressed sympathy for squatters. Thomas Dwight to Theodore Sedgwick, April 21, 1798, Sedgwick Papers, Massachusetts Historical Society. But he also took positions that the speculators endorsed. "The Federalist Great Proprietors," Alan Taylor writes, "should have known that they had little to fear from the new politicians, because so many of the leading Jeffersonians used to work for them"; Taylor, *Liberty Men and Great Proprietors: The Revolutionary Settlement on the Maine Frontier* (Chapel Hill, N.C., 1990), 216, see also 23–24.

⁶¹ [James Sullivan], *The Path to Riches: An Inquiry into the Origin and Use of Money; and Into the Principles of Stocks and Banks, to which are Subjoined Some Thoughts Respecting a Bank for the Commonwealth* (Boston, 1792).

ible with a society of individuals exercising free choice.”⁶² He was on the bench when the first in the series of Quock Walker cases was decided in 1781; he believed that public opinion had made slavery untenable in Massachusetts and that the nation as a whole should adopt a long-range program of gradual emancipation coupled with equal education for children black or white.⁶³ Sullivan was skeptical of capital punishment⁶⁴ and an ardent advocate of religious liberty; he defended the Universalists—a high proportion of whom were women—both in court and in print at a time when they were very unpopular.⁶⁵

And, most important for our purposes, Sullivan was willing to follow consent theory where it led, certainly as far as the elimination of property requirements for voting and, long before the *Martin* case, up to the boundaries of gender. In the spring of 1776, Sullivan wrote to Elbridge Gerry:

Every member of Society has a Right to give his Consent to the Laws of the Community or he owes no Obedience to them. This proposition will never be denied by him who has the least acquaintance with true republican principles. And yet a very great number of the people of this Colony have at all times been bound by Laws to which they never were in a Capacity to Consent not having estate worth 40/per annum &c . . . Why a man is supposed to consent to the acts of a Society of which in this respect he is absolutely an Excommunicate, none but a Lawyer well dabled in the feudal Sistem can tell.

Sullivan distrusted the rich as well as the poor—“Stupid Souls . . . are as often found on the throne as on the Dunghill”—and urged that voting for legislation that did not include new taxes should involve “every person out of wardship that is bound thereby.”⁶⁶ Gerry sent Sullivan’s long letter on to John Adams, who was representing Massachusetts at the Continental Congress in Philadelphia and daily facing the problem of how to turn the claims of the consent of the governed into practical devices for a new political order. Sullivan’s letter came hard on the heels of Adams’s own correspondence with his wife on this point: “In your new code of laws,” Abigail Adams had written, “I pray you remember the Ladies.” Together, Abigail Adams and James Sullivan forced John Adams to a consideration of the composition of “the public.” In response to Sullivan, John Adams wrote the powerful, now well-known letter of May 26, 1776, in which he contemplated the question “Whence arises the Right of the Men to govern Women, without their Consent?” Sullivan had said that “every person out of Wardship” should partici-

⁶² These words are from Alan Taylor, personal letter to Linda K. Kerber, May 27, 1991.

⁶³ On Quock Walker, see Amory, *Life of James Sullivan*, 1: 115, 120; and John D. Cushing, “The Cushing Court and the Abolition of Slavery in Massachusetts: More Notes on the ‘Quock Walker Case,’” *American Journal of Legal History*, 5 (1961): 121 n. 6, 137. Sullivan’s most extended comments on slavery appear in letters to Jeremy Belknap, April 9, July 30, 1795, in Massachusetts Historical Society, *Collections*, 5 ser., 3 (1877): 402–03, 412–16.

⁶⁴ See essay signed “Marcus” in *Independent Chronicle*, February 7, 1793, and essay signed “Plain Truth,” *Independent Chronicle*, August 2, 1793.

⁶⁵ *An appeal to the impartial public by the Society of Christian Independents Congregating in Gloucester* (Boston, 1795). See also James Sullivan to [?], June 25, 1785, Sullivan Papers, Massachusetts Historical Society.

⁶⁶ Sullivan to Elbridge Gerry, May 6, 1776, in *Papers of John Adams*, Robert J. Taylor, et al., eds. (Cambridge, Mass., 1979), 4: 212–13. Sullivan had read his Paine: “Government is founded on the Authority of the people, and by them only is Supported and is as the writer of Common Sense observes, not founded so much in human Nature, as in the depravity of it.” He suggested a two-tier voting system that took relative wealth into account.

pate in legislation in some way, and Adams might well have responded that coverture placed married women in “wardship” to their husbands. But Adams construed Sullivan’s letter to imply that women and children were governed without their consent, and he could see that the logic of liberalism, pressed as extensively as Sullivan was willing to press it, led inexorably to a challenge to coverture. “Depend upon it, sir,” Adams warned, “it is dangerous to open So fruitfull a Source of Controversy and Altercation, as would be opened by attempting to alter the Qualifications of Voters. There will be no end of it. New Claims will arise. Women will demand a Vote.”⁶⁷

Sullivan continued to align himself with those who supported various challenges to women’s subordination. Kathryn Kish Sklar finds him active in 1792, as state attorney general, persuading a jury “that girls had equal rights under the constitution and could not be expelled from school” when the selectmen of the town of Northampton refused to allocate funds for the schooling of girls.⁶⁸

In short, Sullivan’s role in the *Martin* case was congruent with the unusually consistent liberalism he displayed throughout his career. Believing that society was composed of equal individuals, he spun out the implications of that belief in a wide range of issues as they presented themselves—banking and the economy, religious freedom, an end to slavery, and even gender relations.

AS THEY FACED EACH OTHER IN COURT, each of the attorneys and judges had a high—and, in the cases of Dana, Sedgwick, Sullivan, and Parsons, formidable—political profile. Two men had actually served in the General Court (Dana on the Council, Sullivan in the House) when the confiscation legislation was passed in 1779. The strategy of the attorneys who spoke for Martin was to confine the case to narrow grounds. Had the Court of Common Pleas had appropriate jurisdiction? Had there been procedural due process? Was William Martin’s title clear? The strategy of the two opposing lawyers, Davis and Sullivan, explicitly located the case in its revolutionary context, defended the choices of the revolutionary era legislature, and urged the court to sustain the radical implications of that legislation.

As the case was tried, the argumentation developed along two lines. One, whether the Martins had been granted due process of law, was relatively free from gendered considerations. I will not try to reconstruct it in this essay, but I will note that, while Martin’s lawyers emphasized claims to due process, James Sullivan introduced a political dimension by claiming that a loose interpretation of statutes was permissible in time of war. The other line of reasoning challenged the relationship of a married woman to the state.

No one denied that if William Martin had possessed property, the state had a right to seize it. Martin had met virtually all the requirements of the confiscation statute: as an officer in the Royal Regiment of Artillery, he had levied war against the government and people of the United States; he had withdrawn from

⁶⁷ Adams to Sullivan, May 26, 1776, in Taylor, *Papers of John Adams*, 4: 208–13.

⁶⁸ Kathryn Kish Sklar, “Popular Sources of Change in the Schooling of Girls in Massachusetts, 1750–1820,” unpublished paper, c. 1990, cited with permission of the author.

Massachusetts to a place “under the power of the fleets or armies of the . . . king,” and he had not taken “an oath of allegiance to [the United] . . . states.”⁶⁹ No one denied that Anna Martin possessed property. She, too, met the explicit conditions of the statute: she, too, had absented herself from the state of Massachusetts after April 19, 1775. Did the state have the right to seize her property as well? On what terms might it make such a claim?

The answer would be found, George Blake argued in his opening statements, in a close reading of the text. His voice reaches across the centuries to make explicit what had always been encoded in the concept of *feme covert*. The Confiscation Act addressed itself to “every *inhabitant* and *member* of the state.” Women were inhabitants of the state; were they also members? Blake thought not. “Upon the strict principles of law, a *feme covert* is not a member; has no *political* relation to the *state* any more than an alien . . . The legislature intended to exclude *femes-covert* and infants from the operation of the act; otherwise the word *inhabitant* would have been used alone, and not coupled with the word *member*.” As we have seen, confiscation had been linked legislatively with oaths of allegiance and with definitions of treason. Blake drew on these oaths and definitions for analogies by which to maintain his point: “A *feme-covert* was never holden to take an oath of allegiance.” Like treason statutes, he went on, the confiscation “statute is highly penal” and therefore demanded very narrow interpretation. When Blake read the preamble, he construed it in gender-specific terms:

“Whereas every government hath a right to command the *personal services* of all its *members*, whenever the *exigencies of the State* shall require it, especially in times of an impending or actual *invasion*, no *member* thereof can then withdraw *himself* from the jurisdiction of the government, and thereby deprive it of *his personal services*, without justly incurring the forfeiture of all *his* property, rights and liberties holden under and derived from that constitution of government, to the support of which *he* hath refused to afford *his aid and assistance*” . . . The object [of the preamble] was not to punish, but to retain the physical force of the state . . . How much physical force is retained by retaining married women? What are the *personal services* they are to render in opposing *by force* an actual invasion? What *aid* can they give to an enemy? So far are women from being of service in the defence of a country against the attacks of an enemy that it is frequently thought expedient to send them out of the way, lest they impede the operations of their own party.⁷⁰

Blake proceeded to run through a long list of British precedents, easily found, for under the common law rule that “*as a woman is supposed to have acted under the coercion of the husband*,” she is regularly excused for acts, otherwise illegal, committed with him.

“And can it be supposed”—one imagines him thundering—“in the case before the Court, that the legislature contemplated the case of a wife withdrawing with her husband? It ought not to be, and surely was not, intended that she should be

⁶⁹ “An Act for Confiscating the Estates of Certain Persons Commonly Called Absentees.”

⁷⁰ *Martin v. Commonwealth*, 362–63, emphasis in original. The governor of North Carolina had received a plea to permit female absentees to claim dower right in confiscated property even if they left the state. Samuel Johnson wrote at the end of the war, “their return to the Country would have added very little to the Strength of our defense nor were they in any instance bound to find Substitutes.” Johnson to Governor Adeneas Burke, April 1, 1783, in Clark, *State Records of North Carolina*, 16: 953–54.

exposed to the loss of all her property for withdrawing from the government with her husband. If he commanded it, she was bound to obey him, by a law paramount to all other laws—the law of God.”⁷¹

Blake’s argument is important because it spelled out, in politically intense and loaded language, what his contemporaries took to be the political implications of the concept of the *feme covert*. As inhabitants of the state, women were merely residents. Only men were members of the commonwealth; indeed, the eighteenth-century euphemism for penis underscored the gendered implications of the contemporary concept of the “citizen”: at some level, to be a “member” was necessarily masculine and generative. Implicit in this understanding was the antique definition of citizen, a definition as old as the Roman republic: the citizen was the man who is prepared to take up arms to defend the republic and so, in reciprocal relationship, has a right to claim a voice in the decision to resort to arms.⁷² Thus Blake could speak in terms of “personal services” and the right of the state “to retain the physical force of the state.” Once this was established, much else followed; at its extreme, the *feme covert* “has no *political* relation to the *state* any more than an alien.” It would be hard to be more direct than this. Many years later, claiming a political voice for women, the abolitionist Angelina Grimke would ask plaintively, “Are we aliens because we are women?” Blake’s answer, obviously, was yes.⁷³

In a striking example of how contemporary discourse shapes the thoughts it is possible to think, no one in the courtroom was able to separate women from infants. Blake could not imagine that by “inhabitant and member” a state might have wished to identify adults who were competent to make judgments of their own, that infants might be inhabitants but not members, that married women might be both. Blake elided the issue by simple assertion: “The legislature *intended* to exclude *feme covert* and infants . . . otherwise the word inhabitant would have been used alone.” Even in his own terms, this was not an accurate assertion, for, earlier in his argument, Blake had given the Treason Act of 1777 a close reading for other purposes, and he surely had cast his eyes on the opening words of that document, which began by saying that every *person* was an “inhabitant and member” of the commonwealth. If women could not be separated from infants, neither could women and infants be separated from men in the revolutionary definition of “inhabitant and member.” But Blake was speaking a quarter-century after the revolutionary statute, and it was becoming possible to construe it in other ways.

IN WHAT WAS, SO FAR AS I CAN TELL, an unprecedented argument, the attorney general and the solicitor general for the state of Massachusetts undertook to challenge what Blake offered as the common sense of the matter. They insisted that Anna Martin met the clear terms of the law: she had withdrawn from the

⁷¹ *Martin v. Commonwealth*, 364.

⁷² J. G. A. Pocock summarizes this relationship in *The Machiavellian Moment* (Princeton, N.J., 1975), 90.

⁷³ Angelina Grimke, *Appeal to the Women of the Nominally Free States* (Boston, 1838), 19.

state. Was she excused because she was *feme covert*? The statute said "any person." The provisions that confiscated dower provided that dower would not be confiscated for women who stayed, suggesting that married women had the "power of remaining or withdrawing, as they pleased." This element of choice, of course, had traditionally been absent from the repertory of the married woman. To introduce it was a challenge to traditional discourse. It was indeed possible to think what we have heretofore assumed to be unthinkable. James Sullivan attacked Blake's assertion that women were not included in clauses phrased with masculine pronouns; that is, attacked Blake's denial of the generic he: "almost all the provisions of the act are masculine." On this point, Sullivan was outraged:

The same reasoning would go to prove that the *Constitution* of the Commonwealth does not extend to women—secures them no rights, no privileges—for it has no words in the feminine gender: it would prove that a great variety of crimes, made so by statute, could not be committed by women, because the statutes had used only the words *him* and *his* . . . Surely a *feme-covert* can be an inhabitant in every sense of the word. Who are members of the body-politic? are not all the *citizens*, members; infants, idiots, insane, or whatever may be their *relative* situations in society? Cannot a *feme-covert* levy war and conspire to levy war? She certainly can commit treason—and if so, there is no one act mentioned in the statute which she is not capable of performing.

Together, Davis and Sullivan articulated a case for the politicized married woman. Shaking loose of traditional assumptions about women's vulnerability, their incompetence, their distance from issues of concern to the commonwealth, Davis and Sullivan offered the court a woman who had been redefined as a competent citizen by revolutionary legislation and challenged to make her own political choices in the crucible of revolution.⁷⁴

Sullivan's language was politicized; it was part of a discourse of revolution. He accused the other side of being unfaithful to the intent of revolutionary legislation: "If all the decisions which were had during war . . . are in time of peace, liable to be reversed, there would be instant cause of war, and there would be no end of war." For Sullivan and Davis, the revolution had claimed the loyalty of all *persons*, defining them as "inhabitants and members" of the state and broadening the obligations of citizenship to stretch past physical service and include the emotional and mental act of allegiance.⁷⁵ Women could share this sort of citizenship, and, they concluded, women could also share its obligations. They leaned on natural law more than common law; indeed, they abandoned the common law principle of "unity of person" accomplished by marriage and substituted a natural law understanding of the possibilities of reason to shape political behavior. To this effect, Davis cited Jean Jacques Burlamaqui on *The*

⁷⁴ *Martin v. Commonwealth*, 362, 375–76. Sullivan's position was, indeed, congruent with the traditional understanding that the husband who abjured the realm was civilly dead, leaving his wife as *feme sole* and thus vulnerable to the direct claims of the state. See, for example, *Laws Respecting Women*, 171; Dane, *General Abridgement and Digest of American Law*, 335; Reeve, *Law of Baron and Feme*, 98–105. I am grateful to Hendrik Hartog for this observation. In these cases, however, it was assumed that the husband (but not the wife) had left the realm, and that he had surrendered his claim to the body of his wife.

⁷⁵ He might here have cited the Treason Act of 1777, which had specified "All *persons* . . . owe allegiance and are members of the State"; "An Act Against Treason and Misprision of Treason."

Principles of Natural Law: "Reason being the first rule of man, it is also the first principle of morality, and the immediate cause of all primitive obligation." It may be that the woman Sullivan and Davis envisaged bore some debt to Mary Wollstonecraft's construction, which offered competence and capability as preconditions of citizenship. "How can a being be generous who has nothing of its own? or virtuous who is not free?" Wollstonecraft had asked. If Anna Martin were to be a member of the commonwealth, she would have to be defined as a being who had something of her own.⁷⁶

THE END OF THE STORY IS QUICKLY TOLD. The politics of the courtroom pitted the republican candidate for governor against a panel of judges committed to his political defeat. The two days of argument took place before a bench composed of men in late middle age—Thatcher, the youngest, was fifty-one; Dana, the eldest, sixty-two—who had moved in and out of elective politics.⁷⁷ All four judges voted to support James Martin's claim to his mother's property, thus reversing the lower court's decision. Three addressed the issue of the nature of female citizenship in a republic; the most extensive of these was offered by Theodore Sedgwick.

Sedgwick had been a cautious supporter of the revolution and now was a conservative Federalist.⁷⁸ In his written opinion, he began with the revolution, offering a curiously apolitical interpretation of what it had been, stressing its elite origins and respecting those who from "principles of duty and conscience" could not support it. Rereading the charges, Sedgwick emphasized that William and Anna Martin had been *jointly* charged with levying war, adhering to the king of Britain and withdrawing themselves. "[W]e are called upon . . . to say whether a *feme-covert*, for any of these acts, *performed with her husband*, is within the intention of the statute; and I think that she is not." The common law, Sedgwick argued, exempts a married woman from punishment for most crimes when performed with her husband because of the strength of his "authority" and "her duty of obedience." How can we have a situation, Sedgwick asked, in which women are not held responsible for independent judgment on straightforward ethical matters like theft and yet would be held to independent judgment on political

⁷⁶ J. J. Burlamaqui, *The Principles of Natural and Politic Law*, 5th edn. (Cambridge, 1807), 149; Mary Wollstonecraft, *Vindication of the Rights of Woman*, Miriam Brody, ed. (1792; London, 1985), chap. 9, p. 259.

⁷⁷ Sedgwick, Thatcher, and Dana were formally identified with the Federalist Party; Simeon Strong had been briefly arrested as a loyalist during the revolution and subsequently confined himself to the private practice of law. He was appointed to the bench by Federalist governor Caleb Strong in 1801. See William T. Davis, *Bench and Bar of the Commonwealth of Massachusetts* (New York, 1974), 246; Gawalt, *Promise of Power*, 48, 69; Shipton, *Sibley's Harvard Graduates*, 93–95.

⁷⁸ Sedgwick had held this position for a long time. As early as 1778, he had, in correspondence with his Loyalist friend Henry Van Schaack, described Tories as "Persons who have done no more than in meer opinion to dissent from the rest of the Country." Theodore Sedgwick to Henry Van Schaack, August 24, 1778, quoted in Maas, "Return of the Massachusetts Loyalists," 402 and 422 n. 38. As a member of the General Court in the early 1780s, Sedgwick had voted against harsh treatment for returning Tories. Maas reports that Sedgwick enthusiastically arranged for the return of Peter and Henry Van Schaack, even to the point of bribing the justices of the peace; 444 and following. See also Gawalt, *Promise of Power*, 48–49.

matters in which even "men of great powers and equal integrity . . . [are] divided"?

Can we believe that a wife for so respecting the understanding of her husband as to submit her own opinions to his, on a subject so all-important as this, should lose her own property, and forfeit the inheritance of her children? Was she to be considered as criminal because she permitted her husband to elect his own and her place of residence? Because she did not, in violation of her marriage vows, rebel against the will of her husband?⁷⁹

In this way, Sedgwick came to articulate the issue of the relationship of women to rebellion and disorder. Did the state intend—was it possible to imagine the revolutionary coalition in Massachusetts as having intended—to call upon married women to rebel against their husbands? It had certainly called upon sons to rebel against their fathers. But, Sedgwick thought, the state could not possibly have tried to recruit women; there was nothing they could contribute to the rebellion.

A *wife* who left the country in the company of her husband did not *withdraw* herself—but was, if I may so express it, withdrawn by him. She did not deprive the government of the benefit of her personal services—she had none to render—none were exacted of her . . . Can any one believe it was the intention of the legislature to demand of *femes-covert* their *aid and assistance* in the support of their constitution of government?

Sedgwick proceeded to reconstruct the preamble, which defined it as the duty of all the "*people* . . . to unite in defence of their common freedom, and *by arms* to oppose the fleets and armies of the said King; yet, nevertheless, divers of the *members* of this [state] . . . evilly disposed . . . did withdraw themselves." He asked:

[C]an it be supposed to have been the intention of the legislature to exact the performance of this duty from *wives* in opposition to the will and command of their husbands? Can it be believed that a humane and just legislature ever intended that wives should be subjected to the horrid alternative of, either, on the one hand, separating from their husbands and disobeying them, or, on the other, of sacrificing their property? It is impossible for me to suppose that such was ever their intention.

In the end, all four judges chose common law over natural law, English precedent over republican potential, narrow interpretation over loose construction. They chose James Martin's private claim over the public claim of the Commonwealth of Massachusetts. The judges spoke in terms of deference, of obligation, of what women owed to their husbands, what men had a right to demand of their wives. Dana spoke of the "duty, which, by the laws of their country and the law of God, [women] . . . owed to their husbands"; Sedgwick spoke of the propriety of women's submission to her husband's opinions and judgments, even when they were evil; Strong observed that the married woman "is bound to obey his commands . . . except *perhaps* in treason and murder."⁸⁰

The marriages these men describe are not companionate marriages. The women they describe are not "republican wives" in Jan Lewis's terms; they

⁷⁹ *Martin v. Commonwealth*, 392–93, 391.

⁸⁰ *Martin v. Commonwealth*, 390–92.

certainly do not have the freedom to be republican mothers.⁸¹ Theirs is a language of constraint, of control and of force. Sir William Blackstone had defended coverture as protective; the woman, he said, was a “favorite” of the laws of England. But there is little in the language of the judges or of the plaintiff’s attorneys that suggests favoritism and protection: all is force, violence, constraint. In odd juxtaposition, the Federalists spoke of the revolution—on the face of it, a locus of force and violence—in mild terms. They referred to the revolution primarily in connection with giving extended time for loyalists to make up their minds about allegiance, in referring to the persistence of English common law despite the revolutionary fault line. To construct a married woman this way in the context of the *Martin* case was to deny the claims of the American citizens—the purchasers of the confiscated property, as well as the state of Massachusetts—and to privilege the claims of the alien and Tory; yet this conclusion was reached by all the judges. The paradox was that, in order to sustain the state’s claim that Anna Martin had been a “member” of the Commonwealth, she and her heirs would have had to forfeit their property.⁸²

Federalist judges ruled on behalf of the Tory claimant as they had often done in the past.⁸³ The significance of the *Martin* case lies not so much with the substance of the decision, which was congruent with long-term trends in Massachusetts and elsewhere, but in the texture of the argument. The Federalist jurists rallied around a traditional, corporate vision of society in which the family was still a “little commonwealth” headed by a benevolent patriarch. Ironically, they may have been blinded from understanding how restrictive their decision was precisely because they understood themselves to be personally generous, even indulgent, toward the women of their own families. Sedgwick took enormous pride in having freed the family’s slave, “Mumbet,” who took the name Elizabeth

⁸¹ Jan Lewis, “The Republican Wife: Virtue and Seduction in the Early Republic,” *William and Mary Quarterly*, 3d ser., 44 (1987): 689–721.

⁸² The *Martin* case is unusual for the clarity with which the matter of women’s status was discussed. There are some similarities between it and *Lessee of Pemberton v. Hicks*, 1799, in Horace Binney, *Reports of Cases Adjudged in the Supreme Court of Pennsylvania* (Philadelphia, 1809), 1: 1–24, in that the property of the wife of a loyalist was also at issue. But the facts of the case were significantly different. Joseph Galloway had not only been a loyalist but an attainted traitor, who had fled to England. He had held property “in right of his wife,” Grace Galloway, who, like Anna Martin, had inherited land from her father, which Joseph held as tenant by courtesy. But Grace Galloway had met the requirement that she stay in the United States in order to maintain her claim on the property. As a traitor, Joseph Galloway was regarded as “civilly dead,” thus all rights reverted to her, and for civil purposes she was treated as a widow. When she willed the property to her son-in-law, the Supreme Court of Pennsylvania upheld her claim. Yet, even in this lawsuit, one of the judges who voted with the majority made the point that he was “averse to visiting the sins of the father upon the children”; p. 12. The dissenting judge argued that the property had been properly confiscated when Galloway was attainted, since the purpose of deterring treason would not be served if the property remained in the family. “The only human consideration which can withhold [traitors] . . . from endangering the nation, is their attachment to their wives and children . . . Besides, property is created and preserved by government and laws; consequently every government may regulate it in such a manner as the society deems most conducive to the good of the nation”; p. 18. Judge Smith was, like Sullivan, willing to sustain the radical purposes of revolutionary legislation, but the status of Grace Galloway did not enter into the argument.

⁸³ David Maas finds that, with a few brief exceptions in the immediate postwar period, Tory claimants won more cases than they lost; Maas, *Divided Hearts: Massachusetts Loyalists 1765–1790; A Biographical Directory* (Boston, 1980), xxiii. See also Maas, “Return of the Massachusetts Loyalists,” 524–25 *et passim*.

Freeman, and in nurturing his own daughter's love of reading—Catharine Maria Sedgwick, fifteen years old in 1805, who grew up to be a leading novelist of her generation. Yet that personal generosity itself left patriarchal relations intact; the father who chose to be indulgent toward his dependents had the option of choosing otherwise.⁸⁴

That conservative public politics should be thus linked with patriarchal private relations comes as no surprise, except insofar as these Federalist judges and lawyers are unusually explicit in their denial of the civic capacity of women. They were voicing an interpretation of marital relations that we now understand to be anachronistic in light of what would follow, but they did not have the benefit of hindsight. The state predictably assumed the defense in order to sustain its own claims to the confiscated property. What is intriguing is that James Sullivan and Daniel Davis should have offered the arguments they did in favor of the civic capacity of women. Sullivan and Davis moved far beyond the claim of mutuality in marriage to a vision of marriage in which the partners were independent moral actors, a vision of family life in which wives as well as husbands had to evaluate the revolution, take a position, and risk their property and prosperity on the choice they made. Sullivan and Davis argued that if patriarchy in politics is rejected, so too must be patriarchy in marriage. The Federalist judges wanted it both ways—to abandon patriarchy in politics but maintain it, in sentimental form, in their private lives. Many judges clung to this position for the rest of the nineteenth century; the longest-lived aspect of the pre-revolutionary past would be, Michael Grossberg argues, “judicial patriarchy.”⁸⁵

Sullivan's comments in the *Martin* case suggest the outer limits of arguing the Woman Question. The seizure of a married woman's property because she had not remained in the republic and made her own political commitment was as far as one could go. In Sullivan's language, the insistence that married women had the power to make choices was associated with a vision of the revolution as violent. It was congruent with other positions Sullivan took throughout his career and with positions others articulated—briefly—in the radical spaces of the revolution. Sullivan was consistent in his liberalism, supporting a free market not only in the economy but in ideas, not flinching when that position suggested that women were necessarily part of the polity. No one joined Sullivan and Davis out on their limb, and they did not venture out there again.⁸⁶

⁸⁴ See Catharine Maria Sedgwick, *Hope Leslie: Or, Early Times in the Massachusetts*, Mary Kelley, ed. (New Brunswick, N.J., 1987), ix–xxi. I owe this paragraph to conversations with Alan Taylor.

⁸⁵ Grossberg, *Governing the Hearth*, 296.

⁸⁶ The *Martin* decision was reaffirmed by Theophilus Parsons, in his role as chief justice of the Supreme Judicial Court, in *Esther Sewall v. Benjamin Lee*, 9 Mass. Reports (1812): 363–70. See also Lewis Bigelow, *A Digest of the Cases Argued and Determined in the Supreme Judicial Court of the Commonwealth of Massachusetts from Sept. 1804 to Nov. 1815 . . .* (Cambridge, Mass., 1818), 238.

James Martin subsequently brought four different and successful ejectment actions against holders of property he claimed he inherited from his mother. These cases can be traced in the records of the U.S. Circuit Court, Massachusetts District, Docket Book, vol. 2, 1804–09, Record Book, vol. 2, 1806–11, and accompanying file papers for *James Martin v. John Bosson*, *Martin v. Benjamin Cargyll*; *Martin v. Winslow Parker*; *Martin v. Levi Thayer*; *Martin v. Paul Thayer*, Federal Records Center, Waltham. Blake served as Martin's attorney in all the suits; Sullivan, in his private capacity, was counsel for Bosson and for Cargyll.

There are a few other revolutionary-era cases in which women figure prominently as plaintiffs or

A QUARTER-CENTURY LATER, Supreme Court Justice Joseph Story brought the issue to one kind of closure in his thoughtful and lengthy opinion for the majority in *Shanks vs. Dupont*, decided in 1830.⁸⁷ Story had known many of the principals in the *Martin* case. As a member of the General Court, he had served on a committee that examined the election returns in 1806; though a Republican, he had refused to vote along party lines and had awarded the disputed election to Caleb Strong rather than to Sullivan.⁸⁸ Story made brief reference to *Martin* in his opinion. Although the issue was inheritance rather than confiscation, the *Shanks* case turned on the question of the allegiance of Ann Scott Shanks. Like Anna Gordon Martin, Shanks was born in the colonies, a subject of the king; she married a British officer in 1781 (that is, before the war's end and while American claims to independence were therefore in some doubt) and returned with him to England. Was she to be treated as a British subject toward whose claims for property the United States was neutral, was she to be understood to be a *feme covert* with no capacity to make a political decision, or was she to be considered an American citizen who had renounced her allegiance? What choices did Shanks have the capacity to make as a *feme covert*? Coverture was still the rule in the United States, and Story did not try to undermine it explicitly. He acknowledged that the act of marriage was "the only free act of her life . . . for from thence she continued *sub potestate viri*." But Story did widen the range of choices available to a *feme covert*.

Story argued that "marriage with an alien, whether a friend or an enemy, produces no dissolution of the native allegiance of the wife. It may change her civil rights, but it does not affect her political rights or privileges." Ann Scott had not lost her American citizenship simply by marrying Joseph Shanks. But, Story argued, she did dissolve her allegiance by going "voluntarily under British protection, and adhering to the British side, by her removal with her husband." Two nations, Britain and America, claimed her allegiance, "but they virtually allowed her the benefit of her choice." Story explicitly denied that "her situation as a *feme covert* disabled her from a change of allegiance." He pointed out that the United States had treated as citizens "British *femes covert*, residing here with their husbands at the time of our independence, and adhering to our side until the close of the war." The court concluded that Ann Shanks, whom the British government had never ceased to treat as a British subject, *was* a British subject and

defendants, notably *Rutgers v. Waddington*, New York City Mayor's Court, 1784, and *Kempe's Lessee v. Kennedy*, 5 *Cranch* 173. But none became the occasion for the detailed expression of ideas about the relationship of women to the state or of a debate on what the civic order owed to married women. Elizabeth Rutgers, for example, was a widow—that is, not a *feme covert*—who sued Joshua Waddington, a British merchant, for "forcibly" occupying and using her brewery during the British occupation of New York City. He had done so by virtue of the authority of the occupying British army, and the argument in the court related to the extent to which the independent United States should, in 1784, recognize authority the British had exercised in 1778. Rutgers' status was never in question: "she was an inhabitant of this city," observed the court, "who by reason of the invasion of the enemy, left her place of abode, and . . . hath not since voluntarily put herself into the power of the enemy . . . [she is] a complete Plaintiff." Richard B. Morris, ed., *Select Cases of the Mayor's Court of New York City, 1674–1784* (Washington, D.C., 1935), 302–05 and following.

⁸⁷ *Ann Shanks et al. v. Abraham Dupont et al.*, 3 *Peters* (1830): 242–68.

⁸⁸ R. Kent Newmyer, *Supreme Court Justice Joseph Story: Statesman of the Old Republic* (Chapel Hill, N.C., 1985), 56–57, 401 n. 88.

that her heirs could claim her property under the terms of the Treaty of 1783. Story made a distinction between the "incapacities . . . provided by the common law [that] apply to [married women's] . . . civil rights, and are for their protection and interest," and married women's "political rights [that] . . . stand upon the more general principles of the law of nations." These political rights, he argued, are not undermined by coverture, nor do they prevent married women from "acquiring or losing a national character." In this way, Story's decision in *Shanks vs. Dupont* opened the door for a broader conceptualization of the political capacity of married women. The woman with political capacity was a woman who could choose not only her husband but also her political allegiance. She would become an alien only by an intentional act, not "because we are women."⁸⁹

Citizenship is basic to other claims individuals make on the state or the state makes on them. By pulling the concept of political rights out from under coverture, Story's decision helped markedly in the redefinitions that would have to occur before it was possible to establish the full political identity of American women, and it pointed the way toward the unambivalent claim of women's civic capacity that was voiced in the Seneca Falls Declaration of Sentiments in 1848. But, not long after that claim was articulated, it was attacked. A statute passed in 1855 provided that an alien woman who married an American citizen "shall be deemed and taken to be a citizen." It said nothing about the converse situation, American women who married aliens. American courts were often prepared to argue that a married woman's citizenship "naturally" followed that of her alien husband, especially if she moved abroad. Such women were said to have "suspended" their citizenship. What "suspended" citizenship meant was not clear, but in some cases women who married aliens were indeed treated as aliens. In practice, they might even be stateless. In 1907, this position was written into law; American women who married aliens lost their citizenship, even if they continued to reside in the United States. The Supreme Court upheld the statute in 1915, arguing in *Mackenzie vs. Hare* that by voluntary marriage to an alien, a native-born woman gave up her citizenship and adopted the nationality of her husband.⁹⁰

Not until national women's suffrage was won, and a vigorous national women's lobbying effort was undertaken, were these statutes revised. The process was long, complex, and has still not been fully analyzed. The Cable Act of 1922, which stated that "the right of a person to become a naturalized citizen shall not be denied to a person on account of sex or because she is a married woman," permitted American women who married foreigners to retain their citizenship but only if they married men from countries whose subjects were eligible for U.S. citizenship—that is, not from China or Japan. Even then, American-born women who married aliens were treated as naturalized citizens who would lose their

⁸⁹ *Shanks v. Dupont*, 246–48.

⁹⁰ The best succinct analysis of changes in women's status as citizen is provided by Virginia Sapiro, "Women, Citizenship and Nationality: Immigration and Naturalization Policies in the United States," *Politics and Society*, 13 (1984): 1–26. My paragraph relies heavily on this incisive essay. See also Waldo Emerson Waltz, *The Nationality of Married Women: A Study of Domestic Policies and International Legislation* (Urbana, Ill., 1937), esp. 25–28, 37; Luella Gettys, *The Law of Citizenship in the United States* (Chicago, 1934), esp. chap. 5, "The Effect of Marriage on Citizenship," 111–41; and the recent study by Candice Dawn Bredbenner, "Toward Independent Citizenship: Married Women's Nationality Rights in the United States: 1855–1937" (Ph.D. dissertation, University of Virginia, 1990).

citizenship should they reside abroad for two years. The Cable Act was extended by amendments well into the 1930s, but loopholes remained, and not all of it was made retroactive. As late as the 1950s, some American-born women were denied passports because they had married aliens before 1922.⁹¹

THE EXTENT TO WHICH THE AMERICAN REVOLUTION was radical or conservative remains to be calibrated precisely. The story of *Martin vs. Massachusetts* suggests that the early national period was Thermidorean. Men who had supported the revolution nevertheless undertook in its aftermath to defuse the memories of revolutionary violence and upheaval, to constrain the renegotiation of gender roles, and to limit the political responsibilities of married (by which they actually meant adult) women. They found it impossible to imagine adult women as anything other than wives. They could not separate the sexual monopoly that a man exercised over his wife in marriage from the political monopoly he was understood to exercise over her property. At the height of the French Revolution, an anonymous contributor to a conservative Boston newspaper had offered the epigraph with which this article began, expressing conservative confusion on this point; the potential of female citizenship is dismissed with a linking of a woman of the *polis* to a woman of the streets.

But the story of *Martin vs. Massachusetts* also contains evidence that an alternate scenario existed, also written by men. This alternative acknowledged the authenticity of the republican break with the past, pointed the way to a reconstruction of the relationship of women to real property, and explicitly claimed for women the responsibility of assuming the obligations of citizenship. The important point is not that this path proved too rocky for the revolutionary generation. The important point is that, for a brief moment, it was glimpsed.

⁹¹ Sapiro, "Women, Citizenship and Nationality," 11–15; personal communication from Lawrence Gelfand to Linda K. Kerber, September 24, 1991.

Learning to Talk More Like a Man: Boston Women's Class-Bridging Organizations, 1870–1940

SARAH DEUTSCH

WOMEN'S ORGANIZATIONS IN THE LATE NINETEENTH and early twentieth centuries across the United States expanded both in number and membership among every class and race.¹ Some historians have seen this growth not simply as part of a larger reform impulse but as a distinct alternative politics—a women's politics.² But in seeking to demarcate male from female politics, these historians risk obscuring the interaction that shaped the evolution of the women's organizations and the larger political realm.³

The women's concepts of their public role and their organizations did not remain constant. They partook of a larger historical dynamic involving shifts in urban political structures, education, men's ideas about reform and about the city, and cross-class relations.⁴ The physical and institutional changes in urban life

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¹ See Karen J. Blair, *The Clubwoman as Feminist: True Womanhood Redefined, 1868–1914* (New York, 1980); Paula Giddings, *When and Where I Entered: The Impact of Black Women on Race and Sex in America* (New York, 1985).

² In particular, Paula Baker's influential "The Domestication of Politics: Women and American Political Society, 1780–1920," *AHR*, 89 (June 1984): 620–47, refers to a female political culture. Lori D. Ginzberg, *Women and the Work of Benevolence: Morality, Politics, and Class in the Nineteenth-Century United States* (New Haven, Conn., 1990), 36–37, warns against exaggeration of a women's politics for the antebellum period.

³ See Jennifer Price, "When Women Were Women, Men Were Men, and Birds Were Hats: Gender Roles and the Formation of the Audubon Societies at the Turn of the Century," January 8, 1989, unpublished typescript.

⁴ For changing ideas about the city, see Colin Bell and Howard Newby, *Community Studies: An Introduction to the Sociology of the Local Community* (New York, 1972); Jon C. Teaford, *The Twentieth-Century American City: Problem, Promise, and Reality* (Baltimore, Md., 1986); and Patricia Mooney Melvin, *The Organic City: Urban Definition and Community Organization, 1880–1920* (Lexington, Ky., 1987). On shifts in reform ideas and taxonomies of reform organizations, see also Nathan Irvin Huggins, *Protestants against Poverty: Boston's Charities, 1870–1900* (Westport, Conn., 1971); and Arthur Mann, *Yankee Reformers in an Urban Age* (Cambridge, Mass., 1954), who both include women but without gender analysis; Melvin G. Holli, "Varieties of Urban Reform," in Alexander B. Callow, Jr., ed., *American Urban History: An Interpretive Reader with Commentaries*, 3d rev. edn. (New York,

influenced these women's visions of their own possibilities and the strategies they chose to pursue those visions.⁵

In their original conception, they had shared in a group of oppositional values, now conventionally labeled "woman's culture," which emerged in some middle-class women's organizations in the nineteenth century as family increasingly became a private entity and less a social metaphor and as individual self-interest and large-scale industrial capitalism increasingly dominated the economy and society.⁶ As electoral politics came to embody and represent competing interests, the adherents of this "woman's culture" sought to provide unity and community to hold the fragmented nation together and prevent the permanent cleavages seen in Europe. They posed the cooperative, nurturing values of home as an alternative to the competitive, individualist values of the marketplace and formal politics. They called on the supposedly universal domestic experience of women to strengthen ties across economic, ethnic, and even racial boundaries.

But something happened to this vision as organized middle-class women expanded their public roles. Most historians would agree that by 1920 the older vision had been largely replaced with a common male and female politics reinforced by the achievement of woman suffrage. The transformation would seem to have occurred in the reform decades of the late nineteenth and early twentieth centuries, and it is on those decades that I will focus, with reference to later eras to demonstrate the fulfillment of certain trends.

Historians' understanding of the changing views of organized women from 1870 to 1940 has benefited from a burgeoning literature on women, reform, and the state.⁷ Some historians have referred to the changes as the "domestication of

1982), 210–25; and Robert H. Wiebe, *Businessmen and Reform: A Study of the Progressive Movement* (Cambridge, Mass., 1962).

For some of the many scholars beginning to explore shifts in female reform ideas, see Ellen Fitzpatrick, *Endless Crusade: Women Social Scientists and Progressive Reform* (New York, 1990); Elizabeth Anne Payne, *Reform, Labor, and Feminism: Margaret Dreier Robins and the Women's Trade Union League* (Urbana, Ill., 1988); Kathryn Kish Sklar, "Hull House in the 1890s: A Community of Women Reformers," *Signs*, 10 (1985): 658–77; Jill Kathryn Conway, *The First Generation of American Women Graduates* (New York, 1987); and Nancy F. Cott, "What's in a Name? The Limits of Social Feminism: or Expanding the Vocabulary of Women's History," *Journal of American History*, 76 (December 1989): 809–29. On an earlier period, see Nancy A. Hewitt, *Women's Activism and Social Change: Rochester, NY 1822–1872* (Ithaca, N.Y., 1984), which is unusually sensitive to differences among women's organizations.

⁵ On gender and space, see Dolores Hayden, *The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities* (Cambridge, Mass., 1981); Linda K. Kerber, "Separate Spheres, Female Worlds, Woman's Place: The Rhetoric of Women's History," *Journal of American History*, 75 (June 1988): 9–39; Gunther Barth, *City People: The Rise of Modern City Culture in Nineteenth Century America* (New York, 1980); Christine Stansell, *City of Women: Sex and Class in New York, 1789–1860* (New York, 1986), esp. chap. 10; and Mary P. Ryan, *Women in Public: Between Banners and Ballots, 1825–1880* (Baltimore, Md., 1990).

⁶ Barbara J. Berg, *The Remembered Gate: Origins of American Feminism; The Women and the City, 1800–1860* (New York, 1978); Nancy Cott, *The Bonds of Womanhood: "Woman's Sphere" in New England, 1780–1835* (New Haven, Conn., 1977); Hewitt, *Women's Activism*.

⁷ For example, Baker, "Domestication of Politics"; Linda Gordon, *Heroes of Their Own Lives: The Politics and History of Family Violence* (New York, 1988); the October issue of the *AHR*, 95 (1990), particularly Maureen A. Flanagan, "Gender and Urban Political Reform: The City Club and the Woman's City Club of Chicago in the Progressive Era," 1032–50, and Seth Koven and Sonya Michel, "Womanly Duties: Maternalist Politics and the Origins of Welfare States in France, Germany, Great Britain, and the United States, 1880–1920," 1076–1108. See also Eileen Boris, "The Power of

politics” and have seen the entry of organized women into the public realm as a triumphal progress under the banner of civic maternalism, one that allowed them to bring along, unaltered, their own newly created institutions.⁸ Their public conquest made a separate women’s politics irrelevant. Others have taken a dimmer view and have seen in political cooperation with men the destruction, not triumph, of women’s separate vision and often of the women’s organizations themselves, as they slipped into the mire of cooptation.⁹ The three Boston organizations discussed here provide some evidence for both views, but more important, they point to the range of choices and the middle ground negotiated by the women’s organizations that survived. As women entered the changing but still male-dominated urban milieu, they gained new powers and sources of authority and lost some old ones. Where they chose to enter the public arena, they did not do so unchanged. Indeed, any triumphal parade took its shape from the interaction of their reform ideas with the political and social realities of their environment. In short, middle-class women’s class-bridging organizations and their strategies emerged not in an autonomous female realm but within a complex network of power relations.¹⁰

Three very different women’s class-bridging organizations in Boston will be examined for what they reveal about the transformations of urban life and women’s organizations and how women achieved power in a male-dominated local political arena. The Fragment Society, founded in 1812, the Women’s Educational and Industrial Union (WEIU), established in 1877, and Denison House, a women’s college settlement house begun in 1892, all evoked the universalist ideal of woman’s culture.¹¹ These organizations were participating in

Motherhood: Black and White Activist Women Redefine the ‘Political,’” *Yale Journal of Law and Feminism*, 2 (1989): 25–49; Peggy Pascoe, *Relations of Rescue: The Search for Female Moral Authority in the American West, 1874–1939* (New York, 1990); Robyn Muncy, *Creating a Female Dominion in American Reform, 1890–1935* (New York, 1991); Kathleen D. McCarthy, ed., *Lady Bountiful Revisited: Women, Philanthropy, and Power* (New Brunswick, N.J., 1990); and Linda Gordon, ed., *Women, the State and Welfare* (Madison, Wis., 1990).

⁸ See Baker, “Domestication of Politics”; and Muncy, *Creating a Female Dominion*.

⁹ All of Pascoe’s organizations disappear, for example. Pascoe’s nuanced and sensitive analysis does take into account the “ambiguous relationship to Progressive reform” women’s organizations shared, as they “won the battle to add women’s concerns to government, but lost the ability to choose—and to argue for—those concerns as expressions of women’s values”; Pascoe, *Relations of Rescue*, 191.

¹⁰ In their comparative study of the development of the welfare state, Koven and Michel similarly point out that “female reformers using maternalist arguments alone could seldom compel states to act. They were more likely to be effective when their causes were taken up by male political actors pushing other goals.” All sides invoked “maternalism.” The success of these women in shaping the new public welfare systems, they conclude, “was always qualified by prevailing political conditions”; “Womanly Duties,” 1080, 1107.

I am grateful to Eileen Boris for suggesting the term “class-bridging” to me. I have used “class-bridging” rather than cross-class to acknowledge the middle-class dominance of all these organizations. Such dominance and condescension (notions of uplift), however, were also true of the Women’s Trade Union League (WTUL), an organization most historians do not hesitate to label as cross-class, and one that inhabited one end of the spectrum of class-bridging organizations with charitable societies like the Fragment Society on the other end. The WTUL did make an effort to have its governing board composed of equal numbers of workers and “allies,” which none of these organizations did, although some of them did have “workers” as officers at various times.

¹¹ All three of these organizations still exist, although Denison House has moved from its original site.

what the Fragment Society called “the great social reform which every society like ours is trying to push onward” to fend off “by philanthropic” activity European-style “class hatred.”¹² Today, the conservative Fragment Society may look like a completely different beast from the WEIU or Denison House, but to the women of the late nineteenth and early twentieth centuries, they were variations on an attempt to bridge class differences and arrive at social harmony. These organizations maintained that women of all classes had more in common with each other than had men and that, through shared needs, women better than men could bridge social cleavages. But even the most radical rarely if ever set out to eradicate class difference or fully to incorporate members of all classes on equal terms.¹³ While we should make distinctions among women’s organizations, we also need to recognize similarities that were important to the women at the time. Such similarities help explain, for example, how the same individuals could not only belong to but serve as officers in two seemingly ideologically antagonistic organizations, the Fragment Society and the WEIU.¹⁴

The organizations differed markedly in their strategies and ideologies. They ranged from conservative to radical in their thinking about gender and class and reform in general. By looking at organizations across this spectrum within a single political, social, and economic arena, I hope the meaning of transformations in women’s organizations will become clear. The complex relation between changes in women’s, urban, and organizational politics will be traced through the organizations’ shifting language, ideologies, and strategies, as well as through the impact of those shifts on the organizations’ power in the larger arena of the city and on the women’s ability to sustain their original universalist vision. The way the women’s organizations interacted with men and the city’s economic and political structures forms a crucial part of this history.

BY THE LATE NINETEENTH CENTURY, Boston had assumed its modern outlines. The cramped and crowded elite of Beacon Hill had swooped down on the newly

¹² Fragment Society (hereafter, FS), Box 1, Annual Report, 1896, pp. 4–5.

¹³ The difficulties women faced in their attempts to create effective, egalitarian cross-class organizations are well known. Also increasingly evident are the differences in the organizations of women of different classes, races, and ethnic groups. The focus in this article, however, is not on the difficulties of crossing class or on differences among classes and races. Nancy Schrom Dye, “Creating a Feminist Alliance: Sisterhood and Class Conflict in the New York Women’s Trade Union League, 1903–1914,” 225–46, and Robin Miller Jacoby, “The Women’s Trade Union League and American Feminism,” 203–24, in Milton Cantor and Bruce Laurie, eds., *Class, Sex, and the Woman Worker* (Westport, Conn., 1971); Payne, *Reform, Labor, and Feminism*; Nancy A. Hewitt, “Beyond the Search for Sisterhood: American Women’s History in the 1980s,” in Ellen Carol DuBois and Vicki L. Ruiz, eds., *Unequal Sisters: A Multicultural Reader in United States Women’s History* (New York, 1990), 1–14.

¹⁴ Mrs. George Warren, Mrs. Thomas Mack, and Mrs. George H. Norman were active in both organizations in the late nineteenth century. See FS, Box 1, f. 2, for Fragment Society memberships; Women’s Educational and Industrial Union (hereafter, WEIU), acc. 81–M237, ser. 4, Carton 8, f. 124, “Officers of the WEIU 1898–99”; and f. 12, 3v, “Subscription Members from the Beginning of the Union,” May 1877; and WEIU Annual Reports, 1880s–1890s. Mary Morton Kehew, president of the WEIU, also served as treasurer of Denison House in the late 1890s. Minutes of the Executive Committee, Box 1, vol. 5, 1892–1899, November 3, 1897, pp. 243–44, February 9, 1898, p. 253, February 23, 1898, p. 254, in Denison House Papers, Arthur and Elizabeth Schlesinger Library on the History of Women in America, Radcliffe College, Cambridge, Massachusetts.

land-filled blocks of the Back Bay. Elsewhere, in a brief orgy of annexation, Boston brought into the city Roxbury, Dorchester, and other outlying territories. Impoverished immigrants continued to flock to the North and West Ends but also crowded into the genteel housing stock of the new South End, ambitiously developed but victim to one of the century's many depressions. Rapid physical expansion was matched by the spread of the central business district, pushing the poor still further into noisome streets, dank basements, and alley shacks. It spurred a movement to the suburbs by those workers who could afford to move but utterly failed to erode the understated dignity of the Back Bay and Beacon Hill. So successfully did these elite neighborhoods fend off commercial encroachment that their inhabitants remained anomalously, stubbornly anchored as the rest of Boston swirled about them. Almost half the city's workers of 1880 had moved away a decade later, and many times that number had come and gone in the intervening years. Among those left behind were the city's female majority and the one-third of the labor force composed of women.¹⁵

While the economy grew and the population rose rapidly in these decades, from just over 250,000 to well over half a million, Boston failed to keep pace with the rest of the nation's major cities in either category and slipped further and further behind during the twentieth century. This relative loss coupled with an increasing proportion of immigrant residents (one-third in 1880), recurrent labor unrest, and the increasing visibility of ethnic Irish (a majority by 1890) made the tightly interwed and interbred elite anxious about their future. Henry James, visiting his old stomping ground on the Boston Common in 1904, claimed of passers-by, "No note of any shade of American speech struck my ear . . . [T]he people before me were gross aliens to a man, and they were in serene and triumphant possession." Attempting to fend off these newcomers as they had the commercial district, the elite mustered their forces in the centers of power on Beacon Hill—the state house and city hall. And they used their relatively uncontested power in the state house to try to manage the shifting ethnic balance of power in the city. Over the long run, the trends were clear—the breakdown of Yankee and Irish cooperation, the growth of state power over the city, the centralization of urban executive power, and the Irish assumption of Boston's political power. In the short run, however, between 1880 and 1920, Boston survived shifting coalitions, several activist city administrations, two government reform movements, and two major city charter revisions.¹⁶ It was this un-

¹⁵ Frederic Cople Jaher, *The Urban Establishment: Upper Strata in Boston, New York, Charleston, Chicago, and Los Angeles* (Urbana, Ill., 1982), 25, 96–97; Geoffrey Blodgett, "Yankee Leadership in a Divided City, 1860–1910," in Ronald P. Formisano and Constance K. Burns, eds., *Boston 1700–1980: The Evolution of Urban Politics* (Westport, Conn., 1984), 91–92; Walter M. Whitehill, *Boston: A Topographical History*, 2d edn. (Cambridge, Mass., 1968); Michael P. Conzen and George K. Lewis, *Boston: A Geographical Portrait* (Cambridge, 1976), 12, 35–37; Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973), 16, 25, 40.

¹⁶ Henry James, *The American Scene* (1907; Bloomington, Ind., 1968), 231. See also Jaher, *Urban Establishment*; Alexander Keyssar, "Social Change in Massachusetts in the Gilded Age," in Jack Tager and John W. Ifkovic, eds., *Massachusetts in the Gilded Age: Selected Essays* (Amherst, Mass., 1985), 132–47; Thernstrom, *Other Bostonians*, 25; Blodgett, "Yankee Leadership," 88; Geoffrey Blodgett, *The Gentle Reformers: Massachusetts Democrats in the Cleveland Era* (Cambridge, Mass., 1966); Geoffrey Blodgett, "Josiah Quincy, Brahmin Democrat," *New England Quarterly*, 38 (December 1965): 435–53,

certain world that the three women's organizations negotiated in pursuing their aims.

FOUNDED TO RELIEVE THE POOR, by 1870 the Fragment Society had rejected the dominant ideology of mid to late nineteenth-century reform. Its members referred to themselves as social reformers trying to bridge the chasm between rich and poor, but they remained convinced that the poor existed to test the virtue of the rich in displaying their common humanity and compassion. From the vantage point of Beacon Hill and the Back Bay, Boston's two most elite neighborhoods, they fondly quoted their founding constitution, that it had been "wisely ordained that the poor 'we shall always have with us'" in order for the rich to emulate Jesus by gathering up their fragments and, "anxious to relieve the wants of the destitute," "cast [their] mite on the altar of Benevolence." This benevolence, which usually took the form of bestowing garments, particularly baby sets called layettes, proved them worthy of what they called their "trust," meaning their wealth. It affirmed the rightness of their social and economic position and class distinctions.¹⁷

The members built their society and their discourse on distinctly familistic rather than overtly class lines. Proud of their memberships passed from mother to daughter, and of cousin and sibling relations among members, they modeled their organization well into the twentieth century on the kinship society of Boston's elite neighborhoods.¹⁸ The language of kinship allowed them to ignore the realities of class increasingly visible around them.

It would be easy to caricature the group and their increasing distance from reality. By the 1870s, their sewing circles had become elegant, full dress dinners after which the ladies sat around in their ball gowns and sewed diapers euphemistically called "Art Squares." Such practices could be read as emphasizing class differences rather than fostering class harmony and subjected them to the ridicule of an occasional newspaper article less friendly than the society columns

note that there were over 700 strikes in Massachusetts in 1886 alone; Paul Kleppner, "From Party to Factions: The Dissolution of Boston's Majority Party, 1876-1908," 111-32, and Constance K. Burns, "The Irony of Progressive Reform: Boston 1898-1910," 133-64, in Formisano and Burns, *Boston 1700-1980*; Conzen and Lewis, *Boston*, 12-13, 31; Richard M. Abrams, *Conservatism in a Progressive Era: Massachusetts Politics 1900-1912* (Cambridge, Mass., 1964); Robert A. Silverman, "Nathan Matthews: Politics of Reform in Boston, 1890-1910," *New England Quarterly*, 50 (December 1977): 626-43.

¹⁷ I am grateful to Ann Firor Scott, who first brought this society to my attention. FS, Box 1, f. 1, Marjorie Drake Ross, "A Brief History of the Fragment Society 1812-1962," 1-3; FS, Box 1, Annual Report, 1901, p. 3.

¹⁸ On familial language, see FS, Box 3, f. 14, Minutes 12-9-35, also 4-12-38; Ross, "Brief History," 4, 7; FS, Box 1, Souvenir Yearbook, 1916, pp. 24-25, 31, which points out that the list of officers since founding was short. The membership was more or less consistently 130 to 150 throughout the period. Also, on the same lines, see FS, Box 1, Yearbook, 1923; FS, Box 1, Yearbook, 1925, p. 4: "generation after generation following one another mean much in these changing times." On stability in a rocky world, see also FS, Box 3, 13 v., Minutes, 10-13-73 and 10-16-81. And FS, Box 1, Yearbook, 1934-35, p. 4, two new members of the board had had mothers on the Board of Managers for many years. On the other hand, it may be significant that kinship rhetoric increases in the twentieth-century records of the organization.

of the *Boston Transcript*, which covered their annual meetings. When they finally abandoned the practice of full-dress dinners, it was because of wartime rationing in the 1940s and patriotism, not embarrassment at social inequity.¹⁹ They were unrepentantly an upper-class society.

Their familistic stance, however, also included the roots of resistance to certain aspects of a gender-neutral twentieth-century reform behavior. They disliked impersonal bureaucracy and were determined to retain the Fragment Society's autonomy. In 1865, they had claimed that "the tiny brook which steals almost unseen through the green meadows [is] as really powerful for good, as where, with broader course and sudden plunge, it sets in motion the busy mill." These women conceived, constructed, and perceived of power in terms of the efficacy of personal relations. The almost invisible, natural, nurturing stream (women in their parlors and visiting the poor) claimed transformative power for humans equal to that by which industry (male-controlled) transformed raw materials into finished products by machine. In 1905, the women still perceived themselves a "vital power" in Boston, by use of their traditional methods.²⁰

Well into the twentieth century, these methods included an insistence on personal knowledge of each recipient, not simply for purposes of determining worthiness, although this became of increasing concern with the ascendance of "scientific charity" during and after the Civil War, but for the exercise of the redemptive power of human love and caring across class lines.²¹ The Fragment Society resisted the centralizing, rationalizing pull of scientific charity. They had refused to surrender their autonomy to the Associated Charities in 1879. Led by Robert Treat Paine but under Annie Fields' behind-the-scenes direction, the Associated Charities' motto was "not alms but a friend"; it gave no money, only advice, and eventually it took over Boston's social welfare activities.²² The Fragment Society insisted, by contrast, that quick decisive aid sometimes could not be refused and that "there is this heavenly quality even in a deed of misplaced charity, that it makes the heart of the doer sit lightly in his bosom." They admitted the justice of Fields' premise: "To give education to the children, and lucrative employment to the parents is undoubtedly the best way to administer charity." But, they countered, "to the sick, the aged, and young children, it is impossible

¹⁹ FS, Box 1, Annual Report, 1870, H. L. Brown, secretary, p. 5, already comments on the inattention to sewing. And in Box 2, Minutes, 1883–84, the group decided to move reading scriptures from general to board meetings due to lack of attention. "Dutch Treat" lunches, where each member paid her own way (\$1) began at the WEIU in the 1940s; see FS, Box 1, Yearbook, 1941, p. 13; Ross, "Brief History," 6, and continued through the present day. Compare Blair, *Clubwoman as Feminist*.

²⁰ In that year, they gave away 924 garments, 547 pairs of boots, and 45 baby suits, figures not much changed before or after; FS, Box 1, Yearbook, 1905, p. 3; FS, Box 3, 13 v., Minutes, 10–9–65.

²¹ The religious aspect of the society did not diminish with the increasingly secular nature of the world outside. Bible readings continued to be a regular part of their meetings throughout this period. FS, Box 1, Yearbook, 1916, and similarly 1936–37, 1939, 1941, 1945, 1949, all in Box 1. FS, Box 4, f. 19, indicates that even in 1971 the Fragment Society meeting included a prayer and Bible reading.

²² On scientific methods during the Civil War, see Ginzberg, *Women and the Work of Benevolence*, 133–73. See Fields Addenda, Box 5, draft speeches on Charity, Huntington Library, San Marino, California.

often to refuse direct aid, to meet the wants which those who labor among the poor encounter at every step."²³

This "efficiency" versus "sympathy" debate was part of a larger dispute over the appropriate manifestation of women's supposedly innate benevolent impulse in a wider sphere. It contrasted nineteenth-century woman's culture with ascendant business values; it pitted family against corporation as the key metaphor.²⁴ In 1870, the society had insisted that "the poor do not require merely to have alms flung at them, as we fling food to a starving animal. They have other needs besides those of the body"; money and goods should be given certainly, but "the manner of giving charity" was crucial to the Fragment Society, the pleasant word and smile, "which shall make a poor person recognize that you consider him as of the same flesh and blood with yourself; not born an inferior, not to be regarded as the off-scouring of the earth, but one of God's children, one for whom, equally with yourself, Christ died." Nearly seventy years later, the society reported, "Times have changed . . . and now the City helps these poor unfortunates, but not in such a personal way."²⁵ Unlike more modern philanthropic societies that aimed for rapid turnover in clientele, the Fragment Society continued fondly to mention from year to year those it termed its "pensioners."²⁶ Members retained, in short, their personalist, familistic vision of urban life and found a place for that vision in the city by their continued participation in quasi-public welfare.

The Fragment Society was not changeless. In the twentieth century, its members no longer found their recipients on their doorstep or among their casual acquaintances. The geography of the city had changed, creating greater spatial as well as social distance between classes and ethnic groups. Increasingly, the Fragment Society relied on urban institutions instead of personal knowledge to locate the targets of their beneficence, but they resisted the meaning of that geographical change, continuing to focus, even when working through an

²³ FS, Box 3, 13 v., Annual Reports 1870–71, 1877–78, 1885–86. In 1870–71, heavenly quality in misplaced charity, and comment on increased suspicion as increased foreign component among those helped. On personal nature of FS service, see Yearbooks 1909, 1923, 1925, 1934–35.

²⁴ Annie Fields to Robert Treat Paine, 3–28–84, Robert Treat Paine II Papers, Box 1, Massachusetts Historical Society, Boston. Fields calls for "some radical changes to make the work efficient," to make it a "controlling force"; in short, she declares, "I think we need men for agents." See also Huggins, *Protestants against Poverty*, 61–62, 65–66, 118, 173–74. See also Paul Boyer, *Urban Masses and Moral Order in America, 1820–1920* (Cambridge, Mass., 1978), 279; Roy Lubove, *The Professional Altruist: The Emergence of Social Work as a Career, 1880–1930* (Cambridge, Mass., 1965), 11–49; Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977), 86–87; Ginzberg, *Women and the Work of Benevolence*; Ruth Bordin, *Woman and Temperance: The Quest for Power and Liberty, 1873–1900* (Philadelphia, 1981).

²⁵ FS, Box 1, Annual Report, 1870, p. 4; FS, Box 1, Yearbook, 1939, Annual Meeting, actually referring to tubercular patients the Fragment Society aided through a private t.b. association, p. 6. In the 1880s, there was a brief scientific tendency and a shift in language from brotherhood to trying to "make them help themselves"; see FS, Box 3, 13 v., Annual Report, 1882, p. 7, and 1887, p. 5; an apparent pride in FS "efficiency" in Annual Report, 1880–81, and the Minutes of the 10–9–76 and 10–16–81 meetings. But the dominant line is the one described in the text; see FS, Box 3, 13 v., Annual Report, 1885–86; FS, Box 3, 13 v., Minutes of Annual Meeting, 1879.

²⁶ FS, Box 1, Annual Report, 1870, p. 5; FS, Box 1, Annual Report, 1893, p. 4; FS, Box 2, f. 14, Minutes, 1926–1938, February 1927: Mrs. Turner took over the protegee of Mrs. Lincoln, the mother of whom was the grandchild of the original protegee.

institution, on individual knowledge of the person aided, denying their own isolation.²⁷

Such a personalist vision had limits for grappling with a transformed city, however. The Fragment Society women were frustrated by the difference in scale between their own efforts and the magnitude of urban problems, particularly the 1890s machine politics they saw “degrading” not only the “respectable foreign population, but also . . . the native born American citizen.”²⁸ Recognizing the necessity for social reform but convinced that true social reform could come only through individual salvation, the society greeted each new reform epoch with concern and the occasional warning that “the attempt to alter the form of an old established order generally results in its active decline and destruction.” Unlike the other two societies, they did not celebrate the new century. At the century’s turn, women from Denison House and the WEIU joined a torchlight parade to the State House to celebrate the dawning of a new age, while the women of the Fragment Society remained in their parlors.²⁹ As part of a class that witnessed the waning of its centralized authority and power in municipal finance and politics, they feared not only the future but a government over which they had no control. They saw the danger as structural, but, given their fears, they could muster only an individual remedy.³⁰

Emblematic of its acquiescence in, if not contentment with, the status quo, the women of the Fragment Society challenged no spatial boundaries. They met in parlors, definitively women’s space, the private home’s place for contact with the public. When they escaped these parlors in the 1880s, it was only to move to another proven safe space for their class and sex: Back Bay hotel residences. Then, in the 1920s, the Fragment Society began occasionally to meet in the buildings erected by Back Bay women’s clubs to which some of its members belonged.³¹ The Fragment Society never created its own space. It operated within the confines of its members’ world ideologically and practically.

It is significant that this, the “reform” organization least interested in effecting any change beyond the personal and individual, remained the most thoroughly in women’s hands, retaining female control of its treasury, for example, longer than either of the other organizations and remaining unaffiliated with male-dominated umbrella organizations.³² These women were determined to stay in their own

²⁷ FS, Box 1, Yearbook, 1941, p. 10, which shows the organization providing aid via thirty-seven different charities, hospitals, churches, and branches of the Family Welfare, all to individual cases. They noted the advent of Irish on their rolls with dismay, but they gave to them nevertheless. And not until 1949 did they give up sewing the “flimsy and very stupid Art Squares,” in favor of other garments. FS, Box 1, Annual Report, 1887, p. 4; FS, Box 1, Yearbook, 1949, p. 13.

²⁸ FS, Box 1, Annual Report, 1895, on municipal politics; and Annual Report, 1896, pp. 4–5.

²⁹ FS, Box 3, 13 v., Annual Report, 1897, 11–1–97; and see uncatalogued, unprocessed collection of the Twentieth Century Club at the Massachusetts Historical Society, Boston.

³⁰ See Jaher, *Urban Establishment*, 15–158. FS, Box 1, Yearbook, 1906, p. 4; Yearbook, 1916, p. 29.

³¹ See FS, Box 3, 13 v., Minutes; and Yearbooks for their meeting places. In the 1880s, they began occasionally meeting in the Hotel Vendome; in the 1920s, they met at the Women’s Republican Club frequently and occasionally at the Chilton Club, the College Club, the Algonquin Club, and the Women’s City Club. See also Joni Seager, “Father’s Chair: Domestic Reform and Housing in the Progressive Era” (Ph.D. dissertation, Clark University [Geography], 1988).

³² On finances, see FS, Box 1, Yearbook, 1916, p. 31: “It is really an unwritten law of the Society never to beg for money to meet its debts, for we do not allow ourselves to have them. We do all we can with the income we have, and never spend quite up to our limit, feeling happier to be on the safe

sphere and to keep men's hands off it. They remained on the margins of the charitable structure of the city, resisted the temptation to expand, and feared reform; they challenged neither class nor sexual inequities. They did not make their own spaces, but they did keep the ones they had, and in them they were able, against the tide, to maintain an alternative: an organization not bureaucratic but personal, informal, and familistic.

IN THE 1940s, THE FRAGMENT SOCIETY began to meet in the private lunchrooms of the Women's Educational and Industrial Union (WEIU). Despite the interaction, the groups had little in common ideologically. Founded in an Emersonian glow of transcendentalist unity in 1877, the WEIU determined to assist all women in gaining more economic and intellectual self-reliance. Fragment Society rolls were full of women with publicly distinguished husbands; the WEIU was full of women known in their own right, women who had already broken bounds by joining the professions or having a public writing and speaking career, as did Julia Ward Howe. Of eight original members, three, including the founder of the WEIU, Harriet Clisby, were practicing physicians.³³

From the first, the WEIU confronted the urban phenomena the Fragment Society found alarming: the increasing distance that separated racial, ethnic, and class groups in the city and women from one another, and that made it difficult to maintain a sense of kin-based community or neighborhood. Clisby confessed that she "had seen the loneliness, the poverty, the dreadful *apartness* caused by class feeling." Her dream was "an Institution wherein the needs of *all classes* of women would be met, and which should be held together by the bonds of a love whose effects would be shown in *mutual service* and healthy co-operative activities." Unlike the Fragment Society, the WEIU opened not just its purse but its membership to all women.³⁴

While Clisby reached out in particular to women who worked, she tried to bridge the gulf between "the better born" and other classes rather than close it.

side, and ready for any emergency." The other two organizations were constantly in debt in the service of one or another experiment. Note also the FS's simple organization, no departments and few committees, and the changelessness over these eighty years. All things puzzled over were left to the discretion of the Board of Managers. The continued uninstitutional, unprofessional, unbureaucratic nature of the organization as well as its autonomy stand in marked contrast to the others. See FS, Box 3, f. 14, Minutes, 12–13–38. The relatively small size undoubtedly helped (approximately 150 members by the 1940s).

³³ S. Agnes Donham, "History of the WEIU, Boston," 1955, typescript, 8, WEIU Papers, Schlesinger Library, B-8, v. 1. The three were Dr.s Harriet Clisby, Arvilla B. Haynes, and Mercy B. Jackson. Many members were also in the older New England Woman's Club, to the extent that by the 1890s the club blamed the union for its decline. Many of the early and long-lived WEIU members—Julia Ward Howe, Ednah Dow Cheney, Abby May, Abby Diaz, and Mary Livermore, among others—had close ties with the transcendentalists, having been active in the abolitionist movement, taught at the Concord School, studied with Margaret Fuller, or lived at Brook Farm. See, for example, Mann, *Yankee Reformers*, 11, 22; Blair, *Clubwoman as Feminist*, 77.

³⁴ WEIU Addenda, ser. 2, Carton 1, f. 7. "Organizing of WEIU," extract from Dr. Harriet Clisby's reminiscence, Harriet Clisby, "Some Reminiscences of Australia and America," n.d. (circa 1907), 107–08. Clisby claimed that the YWCA and the New England Women's Club did not meet this need for an unconditional membership and civic activism combined.

Like the Fragment Society, some members of the WEIU used the language of family to elide realities of class. The WEIU's second president, Abby Morton Diaz, explained to Robert Treat Paine that the WEIU aimed for "a social intercourse which ignores the boundaries of sect and caste." Yet she hastened to add, "I do not mean by this that equality is possible. It is not found even in families. Kinship is."³⁵ Despite the WEIU's innovation in open membership and its adherence to an ideology and politics of sisterhood, its vision of social amelioration for women stopped short of a classless society.

But the WEIU's membership—open to all on an equal basis—demonstrated its belief that inequality outside the organization need not impede unity and equality within it. Even though they shared the language of family with the Fragment Society, their definition of family seemed different. The WEIU made an active effort to recruit from the city's diverse constituencies, with mixed success. The largest number of members came from the Back Bay, but Mrs. Louis Brandeis, Josephine Ruffin, and Mary Kenney O'Sullivan (a Jewish woman, a black woman, and a female Irish-Catholic labor organizer) served not simply as members but as officers in the organization. Moreover, Diaz argued that the WEIU consisted not of rich women helping the poor "but [of] all women having needs."³⁶

The WEIU had not rejected the vision of city as community, neighborhood, or family, but it used new means consonant with the changing physical reality of the city in order to realize the vision. Immediately, the WEIU created its own space. It rented rooms. This meeting ground for women, Clisby hoped, would break the barriers isolating working women in the city, keeping them alone and desolate in their individual dwellings. In the 1880s, the WEIU went a step further and purchased rooms and buildings in a central location downtown and opened lunchrooms staffed by member volunteers to provide safe space and inexpensive meals for working and other women in the city's center.³⁷

Historians are now accustomed to think of urban parks and even streets as contested space—ground contested for its uses and meaning by men and women, middle and working classes. The city center, including legislative halls, was also a space where different groups struggled for position and negotiated status and

³⁵ Abby Morton Diaz to Robert Treat Paine, January 28, 1883, Robert Treat Paine II Papers, Box 2, January–April 1883. Diaz finished the sentence, "and there is a race kinship," which I interpret to mean human race, since the WEIU strove for racial diversity in membership. Diaz and Kehew, early WEIU presidents, were both of patrician stock but had unusual engagement with a wider experience. Diaz had to support herself and her two sons from an early age, which she did by teaching a variety of subjects, nursing, cooking, domestic work, and finally writing. Kehew was a major force in organizing Boston's working women into trade unions. See Blair, *Clubwoman as Feminist*, 77–80, 82–90.

³⁶ Donham quoting Diaz, "History of the WEIU," 23. According to annual reports, Ruffin served on the domestic reform league committee in 1908, O'Sullivan was a director from 1895 to 1897 at least and served on several committees as well, and Brandeis was on a standing committee from 1897 to 1898 among other years. All of these women were, in some sense, members of an elite. It is less clear whether they were the sole representatives of their racial and ethnic groups. In the Moral and Spiritual Development Committee's efforts at true ecumenicism, they had difficulty finding a rabbi and a priest to participate in their Sunday services.

³⁷ WEIU Annual Reports, 1886–87, p. 12; 1887–88, on growth of the union making it necessary to hire some workers for the lunchrooms and provide some food, pp. 41–42 (125 to 150 clients were using the six tables daily between 11:00 am and 3:00 pm); 1893–94, p. 37; Donham, "History of the WEIU," 29.

definitions of place.³⁸ The WEIU's building added to this sense of the downtown as a "contested space," for the women did not pick any random spot for their building but chose one in the heart of the male-dominated central city.

In an era when men and women shared few spaces on equal terms, organized groups of women, like the WEIU, took an active part in manipulating the physical shape of the city. The groups were often explicit, as was the WEIU, in their desire to create safe and public space for women. They wanted to create public space where middle-class women could appear without being de-classed and, unlike the commercial emporiums of department stores, which were becoming common at this time, space that women controlled and in which women could determine the dominant values and rules of behavior and exchange. They wanted to create space where working women could appear in public without having their virtue questioned by being "on the streets." There was particular interest in creating lunchrooms where young working women would not be exposed to that perennial lothario, the traveling salesman (à la Theodore Dreiser's *Sister Carrie*). And these women's groups were expansionist in their aims. The WEIU bought second and third buildings, opened lunchrooms and employment offices, clinics and pure milk stations, and, by the turn of the century, turned Boylston Street in downtown Boston into virtually a woman's mile. It was not accidental that such proximity to the corridors of power, the state legislature and city hall, coincided with these women's groups' increased lobbying efforts to change the city and their own condition in other ways.

The strategy of welcoming all women to a safe space worked. In the first year alone, the WEIU grew from 8 to 400 members. Ten years later, it had 1,200. As planned, they worked together to transform themselves and the city. They experimented with a host of services: employment offices for domestic servants, "handicapped" women (often meaning women whose only employment handicap was race or age), and college women, vocational advising, a handicraft workshop, a shop selling women's work on consignment, several lunchrooms, kitchens selling nutritional food, school lunches, social investigations, evening classes, milk distribution, lecture series, and Sunday services. When the WEIU eventually persuaded the city to subsidize and then to adopt several of these ventures, the organization itself became part of the government. By altering the relation between the state and public welfare as well as the definition of public welfare, the WEIU succeeded in effecting some of the changes it sought to increase women's self-reliance.

ORIGINALLY, AN ALLIANCE WITH GOVERNMENT had not been part of the plan. This shift in strategy indicated a new WEIU vision of its purpose and of the municipal government. The WEIU recognized institutionally, as it had spatially, the efficacy of new structures for older ends. Yet the new strategy changed the organization. The necessity of learning to speak the politicians' and businessmen's language

³⁸ See Sklar, "Hull House in the 1890s"; Elizabeth Blackmar and Roy Rosenzweig's current work on Central Park in New York City; Hayden, *Grand Domestic Revolutions*; Ryan, *Women in Public*; Stansell, *City of Women*, 193–216, for examples and ideas of contested space.

worked in combination with other forces, including shifts in women's higher education, to affect the original vision of the organization.

It was the WEIU's creation of its own space that had brought men into the WEIU for the first time. Men seem to have been necessary for credit. While the WEIU ultimately had the same size permanent fund as the Fragment Society, its operating costs were several times larger because of its desire to expand and experiment, and women had little access to the sort of assets that made them good credit risks for banks.³⁹ The desire to expand, given the economic structure of the city, warred with the desire for self-reliance. It required the WEIU to find what historians of imperialism have called "indigenous collaborators"—in this case, elite men—whom they believed they could turn to their own uses. These men would cooperate because they saw such organizations benefiting themselves, in part by the provision of welfare services tied to Yankee reformers instead of ethnic political machines. Looking for ways to provide services in any case, they might with the rise of urban reform movements naturally turn to already functioning institutions: women's organizations. Indeed, women's reform groups tended to sound more like machine politicians than like efficient reformers, because they spoke of their desire to humanize the city and their extensive social welfare networks that could provide, as the machine did, jobs, food, coal, child care, and nursing. Women had, in this sense, created their own machine, and their behavior reflected their knowledge of what potential power this "machine" gave them. This position of useful third party was particularly true of settlement workers who, like the ward boss, mediated between their neighbors and the government.⁴⁰

In the 1880s, elite male reform sentiment in Boston coalesced around the issue of civil service reform. With organization came success. The same year that Boston elected its first Irish Democratic mayor, 1884, reformers and their allies in the state legislature transferred control over Boston's increasingly Irish police from the city to a commission appointed by the governor. The following year, the same forces amended Boston's city charter to shelter budget decisions from the influence of ward-based machine pressures and to centralize authority in the mayor's office, as the lesser of two evils.⁴¹ Women's formal politics also became

³⁹ Donham, "History of the WEIU," 10, 31, 32; WEIU Annual Reports, 1889–90, pp. 12–13.

⁴⁰ See William L. Riordon, recorder, *Plunkitt of Tammany Hall* (New York, 1963), 92–93; Huggins, *Protestants against Poverty*, 177; Robert K. Merton, "Latent Functions of the Machine," in Callow, *American Urban History*, 183; Holli, "Varieties of Urban Reform," 210–25. Robert H. Wiebe, *The Search for Order, 1877–1920* (Westport, Conn., 1967), 171, argues that male reform governments were willing, for example, to take over pure milk stations in the interest of creating a systematic city. It seems to me that they were also interested in outflanking the local machines, over whom they won, for the most part, narrow and infrequent victories at the polls. Bosses in need of new allies when they began to lose districts with settlement houses were not averse to adopting the strategy themselves. See Boyer, *Urban Masses and Moral Order*, 181; and Allen F. Davis, *Spearheads for Reform: The Social Settlements and the Progressive Movement, 1890–1914* (New York, 1967), 174–75. On Josiah Quincy III, reform mayor from 1895 to 1899, who delivered gyms, bathhouses, and playgrounds, see Thomas H. O'Connor, *Bibles, Brahmins and Bosses: A Short History of Boston*, 2d rev. edn. (Boston, 1984), 133.

⁴¹ On male reform politics in general, see Wiebe, *Businessmen and Reform*, 108–09, whose businessmen, as Baker, "Domestication of Politics," has pointed out, called on organizations much like the women's interest groups. See also Holli, "Varieties of Urban Reform," 210–14; Boyer, *Urban Masses and Moral Order*, 155–59; Jaher, *Urban Establishment*, 99; Blodgett, *Gentle Reformers*; Kleppner,

enmeshed in a nativist frenzy. Having received the right to vote for the school board in 1879, in the 1880s many Protestant women participated in the effort to purge the Boston School Committee of Catholic influence. The WEIU had members on all sides of the issue, nativists and anti-nativists, suffragists (who tried to distance themselves from the nativists) and anti-suffrage leaders.⁴² The WEIU could contain such diversity because it did not look to electoral politics for public influence. With the redesigned city government and their new male allies, they had other options.

By 1891, the mayoralty was back in patrician hands. Nathan Matthews, tall, thin, and with a pince-nez, brought reform to city hall. A Harvard graduate, real estate lawyer, trust administrator, and civil service reformer, his social profile was identical to that of the men the WEIU invited in as trustees and treasurers. They too had graduated from Harvard, favored civil service reform, practiced law, belonged to exclusive clubs, and appeared in the *Social Register*. Matthews and these others brought tightened and centralized budget-keeping to city government at the same time they expanded its realm with urban planning, city parks, and the first subway in the United States. They saw urban problems as stemming from inefficiency rather than corruption, and they strove to make the city adhere to what they called business-like methods. Politics, they insisted, had no place in municipal government.⁴³ In the 1890s, under Matthews and more particularly under his successor, Josiah Quincy, the city government began to take an active hand in civic and social reform.

The WEIU responded to this new opportunity. By the 1890s, Darwinian language had displaced ubiquitous transcendentalist quotes from Ralph Waldo Emerson in the annual reports.⁴⁴ "Survival" and "efficiency" replaced "unity" and "self-reliance" as the hallmarks of the organization. At the same time, professionals and college-educated women began to replace the generation of middle-class volunteers influenced by transcendentalism. The new generation of female leaders had attended colleges that taught a male curriculum, had read books on political economy and society that predisposed them to arguments of efficiency and respect for a professionalism in which they could now participate. The professionalism of a Clisby or a Haynes in the 1870s was not the self-conscious professional culture emerging in the 1890s.⁴⁵ The WEIU's 29th Annual Report,

"From Party to Factions," 117; Blodgett, "Yankee Leadership," 90, 99. Many of these reformers had close relations to WEIU women.

⁴² Lois Bannister Merk, "Boston's Historic Public School Crisis," *New England Quarterly*, 31 (1958): 172–99; WEIU Annual Report, 1897–98. For example, the Education Committee chair was Lucia T. Ames, suffragist leader; the associate chair was Mrs. L. F. Deland, anti-suffragist. See also Burns, "Irony of Progressive Reform," 140–41.

⁴³ Silverman, "Nathan Matthews," 626–28, 633–36, 643. The WEIU men in the 1890s included Henry H. Sprague, Samuel Wells, and George Wigglesworth. Information on these men is located in *Who's Who in Massachusetts*, 1940–41; *Who Was Who in America*, vol. 1, 1897–1942; *National Cyclopaedia of American Biography*; *Who's Who in New England*, 1909; *Dictionary of American Biography*.

⁴⁴ For Emerson quotations, see WEIU, B–8, Box 1, f. 1. Annual Reports, 1881–82, pp. 11–19; 1883–84, p. 13. Ginzberg, *Women and the Work of Benevolence*, 133–73, noticed a similar shift during the Civil War in the Sanitary Commission.

⁴⁵ See Haskell, *Emergence of Professional Social Science*; Burton J. Bledstein, *The Culture of Professionalism: The Middle Class and the Development of Higher Education in America* (New York, 1976); Lubove,

in 1908, displayed the change. Instead of an institution governed by women's greatest needs and love, the report boldly pronounced, in the face of a deficit of \$9,500, "The test of the Union's right to ask for support is the test of *efficiency*," "*Has the community got its 'money's worth' of results?*"⁴⁶ In the context of over a thousand social service organizations in Boston, only the fittest would survive, WEIU reports warned. One after another, departments were put on a "business" basis. But the shift had come even earlier. In 1900, the union had considered hiring a full-time, paid president. And, in the labor-restless 1890s, it had changed the name of the assembly room from "Women's Union Hall," which some now found objectionable, to Perkins Hall in memory of a male donor.⁴⁷

As professional workers (who came in before the professional president) occupied committee space and took over planning, donors and members became more distant; they found the WEIU less an outlet for their desire to participate in shaping their world. Turnover increased, and the WEIU worried about the decline in participation of volunteers. Employing over a hundred persons by the early twentieth century and having an employees' organization, the WEIU looked more and more like a corporation.⁴⁸ The need to survive did not dictate these changes. The Fragment Society survived without them. It was the WEIU's drive for change and its desire to play a part in the transformation of the city that required new strategies.

In a world increasingly riven by labor strife, including Boston's 500 strikes in the mid-1880s, the younger generation of the WEIU found its elders' ideas of unity and transcendental reality bewildering, quaint, and irrelevant. They conveyed that sense in their reports of attendance at the lectures of or about aging transcendentalists.⁴⁹ Yet male transcendentalists and social reformers had valued women and fostered heterosocial organizations because they also valued attributes connected with prevailing notions of womanhood: spirituality, intuition, and compassion.⁵⁰ When they favored woman suffrage, they did not do so

Professional Altruist; Conway, *First Generation*; and Barbara Miller Solomon, *In the Company of Educated Women: A History of Women and Higher Education in America* (New Haven, Conn., 1985).

⁴⁶ WEIU, Box 1, Annual Report, 1908, pp. 16–17, my emphasis; see also Annual Reports 1891–92, pp. 22, 24, where each "child of the mother institution" was requested to bring money into the treasury; 1896–97 re the Committee on Food, where the issue of priorities ("efficiency" or aid the providers of the food) was still in doubt; 1897–98, p. 12, where the Handwork Department was put on more nearly a "business" basis; 1910–11, p. 49, indicated the Industrial Departments "must be successful, profit-making ventures, in order to furnish the Union the opportunities it desires for the training of women in the management of business affairs," versus 1887–88, p. 26, Industrial Department claiming that, "unlike business in general, it is carried on for the gain of others rather than ourselves," explaining its fiscal insolvency. In 1898–99, p. 41, they decided that if a consigner could not survive at the commission rate of 10 percent, she was unfit to cope with the "business world as represented in the Handiwork sales-room." Earlier, the focus was on training women to do quality work, market, and sell it, seeing them through weaker periods if necessary, to achieve this aim. The shift to a focus on managerial training was particularly significant, both because of its indication that the union had adopted a business mentality and because the union decreased its efforts to train working-class women and increasingly focused on college women and white-collar occupations.

⁴⁷ WEIU, 81–M 237, Carton 1, Annual Reports, 1891–92, p. 17; 1899–1900, p. 8.

⁴⁸ WEIU, 81–M237, Carton 1, Annual Report, 1890–91, p. 9; and 1897–98, on lack of volunteers. Donham, "History of the WEIU," 2, 99, 112. In 1905, Kehew replaced a large number of volunteers with professional staff; Blair, *Clubwoman as Feminist*, 82, gives 120 as the figure.

⁴⁹ See Julia A. Sprague, *History of the New England Women's Club from 1868–1893* (Boston, 1894), 27.

⁵⁰ See Mrs. John T. Sargent, *Sketches and Reminiscences of the Radical Club of Chestnut Street, Boston*

because they saw men and women as alike but because they thought government would benefit from women's special abilities. With increasing tribute paid instead to efficiency and system, women lost this special intellectual allure. If women now wanted to gain respect and power in realms dominated by men, they would have to learn to speak another language, or, perhaps, men and women would have to learn to speak each other's language.

The older union members did not succumb easily. The women of the Protective Department, a forerunner of legal aid in which women volunteers investigated and settled women's cases (usually claims for wages or payment of services) if they could and took them to lawyers if they could not, resisted tampering with their sense of purpose. Indeed, they would only surrender the organization when the city's official legal aid bureau finally hired a female lawyer in 1920–1921.⁵¹ But the initial challenge appeared earlier. In 1880, one of the male lawyers on whom they relied came to convince them to take a small percentage from what they recovered for claimants:

He said he did not look upon the question from the money to be gained, but the good resulting to the women themselves, that in most of the cases the women would gladly give a Lawyer half of what he could recover, but that most of them were so small that the Lawyer would [not] take them except as a charity. That he did not believe in charity to those who did not need it. That charity in the sense of alms (or time) giving is at best dangerous and requires great care and should only exist in cases of absolute necessity and that in most of the Cases of the Union the women, when money was collected for them could, and often would prefer to pay something, perhaps not the regular rate, but something is always gained by cooperation. He thought the best thing in its favor was that the women would feel that they were helping others, and therefore feel more independent. He believed in helping others to help themselves.

When he had finished, the women of the Protective Department politely thanked him for taking the time, feared that the poor women would think that the union was making a profit from them, and agreed with the lawyer on helping others to help themselves; they tabled the motion for further discussion. Later, meeting without the lawyer, the women stood by an earlier decision that the issue was not charity but justice; the female clients had been wrongfully denied money or services. Justice demanded full restitution, and they continued as before.⁵² Two years later, they resisted another lawyer's suggestion that they take cases contingent on settling them as they thought best. They decided instead that clients had

(Boston, 1880), 28, 41–46, 383–84. See also New England Women's Club Papers, Schlesinger Library, Box 10, f. 40, Mrs. Ednah Dow Cheney, booklet of memorial meeting, for instance, F. B. Sanborn, 6–7.

⁵¹ Donham, "History of the WEIU," 154. In WEIU Annual Report, 1887–88, p. 37, the department mentioned it had retained Miss L. J. Robinson, attorney, a short while before.

⁵² See WEIU, Protective Department, ser. 2, Carton 6, f. 106, Protective Committee Minutes, 1878–1894: 2–23–80 and following; on 3–8–80, by seven to two, they voted "that the Protective Dept. shall give its Labor free to working women." See also WEIU Annual Report, 1878–79, p. 22, where the Protective Department declared, "Our aim is to dispense justice, not to give charity, and we are willing to spend some dollars, if necessary, to restore a sum less than a dollar." On 3–3–79, Miss Sprague suggested that clients pay a percentage to avoid dependence and quoted the Associated Charities. Someone replied that it was not charity and not a present but restitution, and the committee resolved that if necessity required a percentage, it would come from their own treasury.

to be left free to consult someone else if they wanted. Despite its maternalist name, the Protective Department stood firm on rights, not control. But finally, in 1898, even the Protective Department adopted two modifications to make it more “efficient,” including charging a 5 percent commission on all monies collected, which put this department, too, in their language, on a “business” basis.⁵³

THE WEIU'S USE OF “EFFICIENCY” and “survival of the fittest” did not signify a new attitude toward the poor but rather a new standard for judging the success of philanthropic and reform organizations. Persisting in much of its original agenda and continuing to broaden the scope of efforts at municipal reform, the WEIU made the city a better place for women to live as well as equipped women better to live in the city through its vocational guidance, employment bureaus, and consignment sales of handicrafts. But the choice of where to place major efforts and which methods to use was no longer governed only by the members' sense of the greatest need. Efficiency, in this usage, meant concentrating on those projects with results most likely to ensure—or at least not jeopardize—continued support for the organization by those who held political and financial power in the city. When Josiah Quincy became mayor, the WEIU reaped some early benefits of this strategy. Quincy was a pro-woman suffrage mayor in the wake of a resounding defeat for the 1895 Massachusetts woman suffrage referendum, and he was also a brilliant coalition builder. Along with machine politicians and Boston Brahmins, he brought into his administration women from the WEIU and other women named to city commissions.⁵⁴ A decade later, in 1907, the WEIU inaugurated a school lunch program in Boston and, contrary to its usual policy, fended off attempts to take it over. It continued to expend its energies on providing lunches and administering the program with the city's funding because the financial scale of the program and the number of its employees gave the WEIU not only patronage and funds but, according to its officers, legitimacy and stature in the eyes of the businessmen and politicians it needed as allies in its reform agenda. The 1908–1909 Annual Report celebrated “the added weight which the higher volume of business gives the Union in its study and endeavor to cooperate with employers in allied trades. Every step that carries the Union forward toward the ranks of the major business enterprises,” the report continued, “puts it in possession of just so much more power to study business conditions and influence

⁵³ Protective Department Minutes, 4–17–82; WEIU Annual Report, 1898–99, p. 50, where they revealed, “We have found a growing feeling that so much charity work should not be asked of lawyers for nothing, who often themselves had need of compensation, and then that all work is worthy of hire and that too much work is given away for nothing.”

⁵⁴ Blodgett, “Yankee Leadership,” 98; Silverman, “Nathan Matthews,” 640; Marilyn Thornton Williams, “Urban Reform in the Gilded Age: The Public Baths of Boston,” 210–31, in Tager and Ifkovic, *Massachusetts in the Gilded Age*; Kehew served on the Bath Commission in 1896, for example; Blodgett, “Josiah Quincy,” 440; James J. Kenneally, “Woman Suffrage and the Massachusetts ‘Referendum’ of 1895,” *Historian*, 30 (August 1968): 617–33. Quincy's expansion of social service and improvement of municipal labor conditions resulted in an increase in spending during this depression decade of twice the increase in the population (from 1895 to 1905). Burns, “Irony of Progressive Reform,” 136–50.

to command a hearing when it speaks.”⁵⁵ Shifts in language and behavior were driven not simply by socialization and generational shifts but also by the desire for male cooperation and for legitimacy in the eyes of male economic and political collaborators.

In 1909 came another major charter revision, the culmination of two years’ work by the Financial Commission, and the WEIU again responded. The new charter altered Boston’s political structure, eliminating ward-based elections, strengthening the mayor’s office, drastically reducing the size of the city council, and imposing two state-appointed watchdog commissions on city finances and appointments. Between 1900 and 1914, WEIU male treasurers and trustees were also members of the Good Government Association, which dominated the charter revision process and gained a majority of the City Council seats in 1910.⁵⁶ Amid this activity, the WEIU centralized its own political efforts, taking city and state politics and not just expansionist city administrators more seriously. In the Annual Report for 1908–1909, the WEIU leaders commented that all the social problems they investigated in the industrial world led to the state house, the city hall, or the police station. They stepped up their lobbying activities, from 1908 to 1918 uniting all the lobbying efforts of the various branches of the union under one department.

Their increasing involvement with the “state” in the form of municipal and state government replaced the sense of the organization as an alternative space and vision of the city. The new vision was more aggressively integrationist. This political activity provided a pre-suffrage training ground for future female politicians (such as Mary Dewson and Ethel Johnson) who would then become part of the appointive political structure. The union hired such women as professionals or college interns for lobbying efforts or investigations of the conditions of working women, often at the behest of municipal, state, or federal government departments. In this way, the WEIU socialized them into the world of politics and the back room.⁵⁷

By 1911, when the WEIU hired its first professional president, little remained of the Emersonian women’s organization of 1877. Professional workers had

⁵⁵ WEIU Annual Report, 1908–09, p. 36: by this time, the Lunch Department receipts were \$180,000 a year, and it employed 160 workers feeding 4,500–5,000 a day. On the attempt to take it over, see WEIU Annual Report, 1910–11, p. 47; and City Council Minutes of that year. By 1908, in addition, instead of the single lunchroom to serve all needs, the WEIU had separate lunchrooms for the public, the members, and its own employees. WEIU Annual Report, 1907, p. 40. Pascoe, *Relations of Rescue*, 180–83, also found language shifts as women revised their appeals to seek new niches and found the new language helped generate a shift in rationale.

⁵⁶ Blodgett, “Yankee Leadership,” 105–06. WEIU Annual Reports, 1900–14; Burns, “Irony of Progressive Reform,” 148–58, 163, 57 n; Davis, *Spearheads for Reform*, 179. On the Good Government Association, see Abrams, *Conservatism in a Progressive Era*, 144; and Peter K. Eisinger, “Ethnic Political Transition in Boston, 1884–1933: Some Lessons for Contemporary Cities,” *Political Science Quarterly*, 93 (Summer 1978): 217–39. On at-large elections increasing the power of business forces, see Boyer, *Urban Masses and Moral Order*, 281–82. WEIU men included George Crocker and Edmund Billings. Also see Davis, *Spearheads for Reform*, 176–78.

⁵⁷ On lobbying, see Annual Reports, 1903–04, pp. 36–37; 1908–09, pp. 18–20, 27–30. See the papers of Ethel Johnson, Mary Dewson, and Louise Marion Bosworth, all of whom worked as investigators for the WEIU, Schlesinger Library; Donham, “History of the WEIU,” 83–84, 95–97, 107, 115–16, 142–43, 166. Topics of investigation included trade schools for girls, industrial conditions of women and children, and employment agencies.

replaced the volunteers in policy making, and businessmen now sat on the organization's advisory boards. No longer seeking primarily to provide an agency for women's cross-class unity, in 1903–1904 the union had even begun admitting men as associate members. By 1927, so changed was the organization that the author of the fifty-year history distorted founder Clisby's words; she quoted Clisby out of context in order to claim that, all along, the ideal had been an association of "men and women acting together" within the WEIU.⁵⁸

Had success spoiled the WEIU? By the 1920s, it had approximately 5,000 members and 350 paid workers. The choice of Eva Whiting White, head of Elizabeth Peabody House and the first recipient of a college degree in social work (Simmons College, 1907), to be the new head of the WEIU in 1929 was telling. White was a professional; her political style was strictly that of pragmatic insider, not oppositional or female-identified. She had succeeded in working with her ward bosses by refusing to criticize the system of ward politics that survived despite centralized elections, and, although she had campaigned for Herbert Hoover, she enjoyed appointments to federal commissions by both Republican and Democratic presidents. She explicitly rejected the label of reformer for that of educator, which she found less inhibiting politically. It allowed politicians, as one reporter explained it, to "love and respect her, because . . . she never assumed the role of a 'do-gooder.'" ⁵⁹

By the end of the 1940s, the WEIU was still controlled by women and focused on women's issues, but, in the course of pursuing its goals, which had gradually shifted from cross-class sociability and mutual aid to municipal reform, its methods, language, and ideology had shifted dramatically from transcendentalist notions of unity and sisterhood and an alternative to mainstream politics to active cooperation with men, professionalized reform and politics, and an insider, integrated position in the city's structure as a vantage more conducive to the realization of its goals. The shift was rooted in the socialization process of each succeeding generation and also owed something to the aggressive experimentation of the WEIU, present from the beginning. But most fundamentally, in contrast to the Fragment Society, it was driven by the WEIU's desire to change the city and not just individual women.⁶⁰ To change the city, women had to create a new source of power through the institutionalization of a women's organization. Then, armed with their collective might, they entered the larger political arena, found collaborators, and altered the political equation.⁶¹

⁵⁸ WEIU Annual Report, 1903–04, p. 11. For the history, see WEIU Addenda, ser. 2, Carton 1, f. 12; Cornelia James Cannon, *The History of the WEIU: A Civic Laboratory, 1877–1927* (Boston, 1927); and Clisby, "Some Reminiscences," 125–26.

⁵⁹ *Simmons College Review*, n.d., p. 10. Eva Whiting White Collection, Schlesinger Library.

⁶⁰ As early as the 1885–86 Annual Report, the president claimed the object was to change existing conditions; p. 7.

⁶¹ This understanding of power builds on Hannah Arendt, "Communicative Power," 59–74, Jürgen Habermas, "Hannah Arendt's Communications Concept of Power," 75–93, and Michel Foucault, "Disciplinary Power and Subjection," 229–42, all in Steven Lukes, ed., *Power* (New York, 1986).

WHILE WEIU WOMEN HAD BROKEN THROUGH CLASS BOUNDS by including working women, Denison House, in Boston's South End, began as a group composed entirely of professional women with careers in various fields from English literature to economics. And, while the WEIU broke through spatial bounds by creating a women's building downtown, Denison House placed its middle-class residents in a working-class neighborhood in 1892 on the eve of a major depression. It created a refuge at once private and public for women, and it provided a middle-class Yankee female incursion into a working-class immigrant neighborhood defined as off-limits to middle-class women. Imbued with an ideology of common humanity similar to that of the two other organizations, the settlement house was also a concession to the reality of the class and ethnic differences and geography that called it into being. The increasingly rigid frontier between working and middle class, foreign and native born, turned settlement house residents into brave pioneers.

Like settlement house residents elsewhere, the Wellesley professors of economics, Emily Greene Balch and Katherine Coman, and of English literature, Vida Scudder, at Denison House struggled to define a place for themselves in the neighborhood.⁶² Though appalled by the standards of cleanliness and lack of gentility around them, they determined foremost to be "neighbors." Like the WEIU, they saw themselves providing literally "common ground," a physical meeting place for different classes. They did not, in these early years, dictate to the neighbors but facilitated for them, and they opened their living room to the local people. On their neighborly visits in the community, the traditional female method of neighborhood formation, they invited all to visit them.⁶³

Like the WEIU women, they tried a host of experiments, from organizing women's labor unions to offering courses on Shakespeare and Dante. They did their best to meet the neighborhood's demands for child care, which, to their dismay, they found more persistent than interest in poetry. They also tried to outflank the local political machine by imitating its structure with their own political forums, boys' clubs, men's clubs, and reading rooms where men were allowed to smoke.⁶⁴

They found their early experience with organized labor gained them rapid, unwelcome press notoriety, and organizing labor unions ceased to be settlement

⁶² See Sklar, "Hull House in the 1890s"; Davis, *Spearheads for Reform*; Judith Trolander, *Professionalism and Social Change: From the Settlement House Movement to Neighborhood Centers 1886 to the Present* (New York, 1987); and Jane Addams, *Twenty Years at Hull-House* (1910; rpt. edn., Urbana, Ill., 1990).

⁶³ Susan Traverso has written a splendid undergraduate thesis on Denison House: "The Road Going to Jericho: The Early History of Denison House, 1887-1912" (B.A. Honors thesis, Simmons College, 1983); and see *Denison House Papers* (hereafter, DH), Schlesinger Library, particularly Box 1, f. 3-5, which contain daily journals kept by the first resident workers and are richly informative about working-class life in the neighborhood as well as cross-class interaction.

⁶⁴ On organizing women, see DH, vol. 1, Daybook, 1893-94, p. 34, September 22; only with the aid of Denison on p. 59, November 21 and p. 65, November 27; also see p. 103, January 9, p. 117, February 4, and p. 131, March 3. Denison House women became delegates from the garment workers' union to the Central Labor union, and eventually many joined a federal union (Local 5915). On day care, see, for example, p. 20, August 26, 1893, where a woman wants to move closer to a day-care provider. They also dispute the Associated Charities tactics to relieve suffering and need (DH, vol. 5, Minutes of the Executive Committee, 1892-99, pp. 56-57, 8-29-94). Denison House was also active in the formation of a Boston Branch of the Union Label League. DH, vol. 3, Daybook, November 1900-08, p. 120, 1-11-1904.

work. Unions continued to meet on the premises, however, and Denison House residents continued to be members. They seem to have refused to take the advice of Robert Woods, who ran a nearby men's settlement house, South End House, to be more neutral in their attitude toward the labor movement; they never seem to have provided equivalent hospitality to corporate leaders and employers as the *Nation* advised. Indeed, a male treasurer resigned in 1904 because Denison House identified itself too strongly with trade unions. As a result, the board began to pressure the settlement workers to be more discreet, but since, under the terms of the College Settlement Association, over half the board members had to be college women, and it was these college women who were the radicals, the pressure was relatively mild, at least before World War I.⁶⁵

At the WEIU, a college-educated and college-socialized staff and professional women had contributed to the WEIU's shift in strategy. Denison House retained an outsider position in the city's politics for a longer time. Both its radical politics, epitomized by a head resident with pacifist socialist tendencies, and its physical position on the fringes of Boston ensured its marginality. In addition, as a settlement house, Denison House remained committed to a belief in the neighborhood as the essential urban unit, whereas the WEIU focused on the central city.

In its sphere, Denison House inaugurated programs that the city then subsidized and occasionally took over.⁶⁶ In this way, it too became a vehicle for the expansion of municipal government but on a different scale. When the women of Denison House went to lobby the mayor to purchase their gymnasium in the early twentieth century, they came away defeated and overwhelmed by the number of calls on the mayor's purse. As neighborhoods became less powerful in the city's structure, the settlement house paid the price and found the requisite shift in strategy trying. Accustomed to raising their own money from other women in small contributions of less than fifty dollars, they were unprepared for the scale of the city budget and unrehearsed in the language of municipal financial politics.

At the same time, they were not like the Fragment Society, content with their limited resources. Like the WEIU, they dreamed of expanding their geographical reach and influence, of engaging the city in the neighborhood, making the city provide a branch library, a gym, baths, and a community center building, recasting the nature of the state and the role of the state in the neighborhood.⁶⁷ Also like the WEIU, these dreams came at a time of an increasing professionalization of staff. In the end, however, Denison House took a different route.

Denison House's first building, and its second and third, were purchased by a

⁶⁵ See *The Nation*, January 3, 1895. DH, Box 1, vol. 2, 5–7–95; vol. 3, 9–20–04, p. 135; vol. 6, Executive Committee Minutes, 2–9–1904, pp. 79–80, and 3–8–04, p. 85. Later, Vida Scudder investigated the Lawrence strike of 1914 and openly supported the strikers while Woods led much of the settlement community against the strike and in support of the AFL. Traverso, "Road Going to Jericho," 79. Davis, *Spearheads for Reform*, 229, claims that Helena Dudley resigned in 1912 lest her radicalism hurt the house financially.

⁶⁶ For example, their manual training classes. See DH, vol. 6, Directors' Meeting Minutes, 3–14–10, p. 140.

⁶⁷ On involvement with the city: DH, vol. 7, Directors' Meeting Minutes, 9–22–08, p. 78; 12–7–08, pp. 88–90; 1–18–09, p. 92; 9–30–09, p. 114; 10–25–09, p. 124, each of which displays a greater sense that it is the city's and not Denison House's job to provide such services.

woman, Cornelia Warren, who remained a force in the house until after World War I. Until 1910, the women had seen no need to bring men into the organization. By then, however, they wanted more. Determined to raise funds to build a new building and achieve more influence in the city, Denison House in 1910 decided to organize a men's auxiliary of influential and wealthy Bostonians. Physically marginal, and without the numbers of wealthy and influential women who graced the WEIU, they hoped an auxiliary of prominent men would give them access to the centers of financial and political power in the city. They successfully, if disingenuously, argued that they had previously concentrated their efforts on the women and girls of the community. In fact, they had always attended to men and boys as citizens and future citizens with greater power to shape the polity, but now they claimed that they wanted to expand their work with boys, to give the plan for a men's auxiliary a sound rationale and incentive.⁶⁸

The balance of the new male auxiliary differed from that of their older male advisory board. In 1903–1904, the board had consisted of a prominent surgeon, a well-known labor leader, and a Harvard-educated Brahmin doctor. The 1910–1911 committee contained no labor representative. It did include Robert Woods and still had two doctors, but among the nine other members, the majority were well-connected lawyers, an architect who became president of the Boston Chamber of Commerce, and a banker who had helped organize the Good Government Association and the Boston City Club.⁶⁹

The ensuing dynamics served as a measure of the disparity of access to political and economic power in Boston by sex. After several meetings of female officers at the settlement house failed to produce promising fund-raising schemes for the building, the women went to see a representative of their new men's auxiliary. He took responsibility, created a list for soliciting, and within months had the sum they had despaired of raising for years. Similarly, after another failed lobbying attempt, the women again approached their new collaborators, who comfortably promised to see the mayor about the issue personally.⁷⁰

The relation of the settlement house to the neighborhood was also changing. The increasingly professional staff had begun to demand their own private living room in 1903, a place to retreat from the “neighbors,” a way to erect a barrier between their professional and private lives. In 1915–1916, the board of Denison House calmly separated the settlement workers' residences from the activities center. Such a notion turned on its head the original concept of a settlement house and the function of the residents: living and working in the community as the

⁶⁸ DH, Directors' Meeting Minutes, 1–13–10, p. 132; 3–14–10, pp. 141, 145; 5–9–10, p. 152; Box 1, vol. 5, Minutes of Executive Committee Meetings, 11–5–95, p. 140, 9–28–05, p. 135; vol. 6, 4–25–05, p. 125.

⁶⁹ Dr. J. E. Goldthwaite, George Edwin McNeil, and Dr. Edward Osgood Otis were on the first committee; the second included Robert Gardiner, Stanley King, Richard Hale, Joseph Coolidge, and Edmund Billings. DH, Box 2, vol. 7, Directors' Meeting Minutes, 1906–11, p. 152, 5–8–10. Information on these men is located in *Who's Who in Massachusetts*, 1940–41; *Who Was Who in America*, vol. 1, 1897–1942; *National Cyclopaedia of American Biography*; *Who's Who in New England*, 1909; *Dictionary of American Biography*.

⁷⁰ It is telling that by 1931–32 there were fourteen boys' clubs and only four girls' clubs. DH, Box 2, f. 8, reports to College Settlement Association. On the building fund, see Directors' Meeting Minutes, for September 1910; on 10–9–11, the women for the first time voiced the hope of being more businesslike in the future; p. 199.

surrounding people did, bridging the gap between classes by common community action based on common residence and ideals of neighborliness. And they were conscious of the shift. In 1912, they baldly reported that the family aspect of the settlement had been outgrown, and Denison House was now a large institution demanding expert workers and adequate salaries.⁷¹ Gone was the original vision of women settling among the poor as true neighbors.

Moreover, the new men's auxiliary, which had seemed so safely marginalized, came with its own price. Adept at raising money, the businessmen simply could not understand what Denison House was trying to achieve when its residents, in the midst of World War I and then a Red scare in 1919, remained outspoken pacifists and socialists. One after another, the men declared that if "the workers could not be restrained from talking in public in a radical manner on questions of the day," the men could not help raise money for the house. In response, some of the women on the board spoke of resigning, and the offending parties also offered to resign. Determined not to sacrifice free speech, even after learning that the federal government had contemplated an investigation of the house, the female board members tried and failed to draft a position paper for the house, and finally decided to ask the men to wait and see how the house worked. Meanwhile, they released them from their obligation to raise money. After additional negative publicity in the *Boston American*, the board asked the head worker to take "a rest."⁷²

This step marked only the beginning of a decline of autonomy in the house and an end to its position as a female settlement house that offered a refuge apart from men for activist women.⁷³ While the next head worker appointed by the board was a woman, she had to divide her time between Denison House and another social work center (over the opposition of Denison House residents), and the head worker who followed her, in the 1930s, was a male. Amid the scarce pickings of the 1930s, Denison House also joined the Community Federation of Boston, a fund-raising organization that required Denison House to promise to begin no new line of work without the advice and consideration of the Community Fund.⁷⁴ Denison House was now thoroughly incorporated into the city's struc-

⁷¹ DH, vol. 6, Executive Committee Minutes, p. 43, 1-23-03, Box 2, f. 8, excerpts from Annual Reports to College Settlement Association, 1915-16; Box 2, vol. 8, Directors' Meeting Minutes, 1-8-12. See also Conway, *First Generation*. Muncy sees the professionalization of social workers as a mixed blessing that received, among women activists, mixed reviews. To some, it was the only way to enter new territories. Muncy, *Creating a Female Dominion*, xiv-xvii, 18, 72-77, 80-83.

⁷² DH, Box 3, f. 9, Directors' Meeting Minutes, 2-10-19, 3-15-19, 1-17-19, 4-21-19, 2-16-20, and Box 4, f. 29, Special Committee Report, 1919, typescript, p. 17, on the displeasure of their Syrian neighbors with their stand on suffrage and prohibition.

⁷³ Denison House had several female "couples" including Katherine Coman who lived with Katherine Lee Bates, Scudder and Converse, Coolidge with her "friend," and others. See DH, Box 3, f. 9, Directors' Meeting Minutes, 7-15-18; Lillian Faderman, *Surpassing the Love of Men: Romantic Friendship and Love between Women from the Renaissance to the Present* (New York, 1981); Judith Schwarz, "Yellow Clover: Katharine Bates and Katharine Coman," *Frontiers*, 14 (Spring 1979): 59-67; Scudder, *Listener in Babel*.

⁷⁴ DH, Box 3, f. 9, Directors' Meeting Minutes, 4-15-20; Executive Committee, Box 3, v. 9, 11-8-35, and 12-17-35. Compare Judith Ann Trolander, *Settlement Houses and the Great Depression* (Detroit, Mich., 1975), on masculinization of settlement houses and on community funds. According to Lubove, *Professional Altruist*, 196. Joseph Lee saw such community federations as pernicious, a way for the businessmen who raised the money to concentrate power in their own hands.

ture. Even though the board still had a female president, women had lost control of the organization as well as their sense of it as a distinctively women's institution.

ASSESSING THE STRUGGLE between Boston's Brahmins and the Irish politicians in this period, Jack Tager concluded, "successful urban government was predicated upon the realities of political accommodation, not upon 'scientific' concepts of efficiency or paternalistic notions of self-improvement." The city's most activist mayors had accommodated both sides of the contest.⁷⁵ The women of these organizations discovered that maternalists, too, had to play the politics of accommodation. They refined and redefined their strategies within a shifting urban landscape.

None of the three organizations could have redefined their strategies with such a relatively free hand had their claims for cross-class unity been realized in their structure, had any of them really attempted equality in governance. The Fragment Society members would have blanched at the notion of working-class women joining their venerable, ivy-laden, tea-stained ranks; they even preferred to help people more like themselves but who had fallen on hard times, women to whom a handkerchief or a slip, given at the right moment, made the difference between self-respect and dissolution.⁷⁶ And the WEIU, though it invited labor organizers and black women onto its committees and governing body, had its committee and executive board meetings at hours when wage-earning women could not possibly have attended, unlike the black League of Women for Community Service, which started as a Soldiers Comfort Unit during World War I and always met in the evenings. The WEIU welcomed the involvement of wage-earning women but tended to set up separate organizations for them, affiliated with the WEIU but not seated on its board.⁷⁷ And while Denison House did ultimately create "a democratic corporation" in 1919 that included local people as voting members if they had contributed money to the settlement house, it did not create a "democratic" governing board but one that boasted instead a majority of college professors and middle-class volunteers and donors.⁷⁸ Moreover, many of the most crucial departments of these organizations shut down

⁷⁵ Jack Tager, "Reaction and Reform in Boston: The Gilded Age and the Progressive Era," in Tager and Ifkovic, *Massachusetts in the Gilded Age*, 244.

⁷⁶ FS, Box 1, Yearbook, 1914, p. 3; Yearbook, 1916, p. 29, quoting 1863 report, also p. 34; Yearbook, 1927, p. 3; Yearbook, 1933, p. 3; Box 3, Minutes, 11–11–38, and Box 2, f. 14, Minutes, 12–1929.

⁷⁷ Probably the best evidence of the failure of cross-class efforts at the WEIU was the organization's obsession with domestic service, how to get women into it, how to train workers, how to train employers, how to make it work. The WEIU Annual Report, 1897–98, p. 16, claimed that domestic service lay at the foundation of social life and threatened its destruction. Other working girls' organizations sponsored by the WEIU included a cash girls' club, which renamed itself as a Junior Workers' Club; Annual Report, 1895–96, pp. 49–50, with some success. There were several forerunners to the WTUL at the time, and Kehew, who became head of the WTUL and was on the board at Denison House, may have brought some of the experience with her. See League of Women for Community Service Minutes, 1918–21, 1924–38, Schlesinger Library, B–L434, microfilm.

⁷⁸ DH, Box 3, f. 9, Directors' Meeting Minutes, 12–15–19. Scudder, who kept trying to teach Dante and Shakespeare to the settlement's neighbors, despite her involvement in the formation of the WTUL, was always more interested in the ex-patriate Italian intelligentsia than in the immigrant Italian peasants. See Davis, *Spearheads for Reform*, 89, for example.

entirely during the summer, their schedules governed by the vacations of the middle-class women who ran them rather than the perennial needs of the working-class women whose allies they purported to be. Individual members of the WEIU and residents of Denison House did seek the collaboration of the labor movement by joining labor unions, federal unions, and even leading the Boston Women's Trade Union League, but Denison House and the WEIU as institutions ultimately sought the majority of their collaborators closer to the center of power in the city.⁷⁹

These organizations had a choice along a spectrum from oppositional to integrationist politics. This choice, however, lay within a larger political structure in which they had little power. When they chose integration, the choice changed them. Ultimately, the integration worked both ways. Intent on effecting political, social, or economic change, Denison House and the WEIU gained access to power through the mediation of influential men, and at the same time they moved away from the original vision of cross-class gender-based unity and the assurance of control by female members of their own organizations. The Fragment Society remained aloof, retaining female autonomy but only within a larger system of limits. It was the conservatism of the Fragment Society that made possible its persistently separate "woman's culture." The price the Fragment Society paid was continued marginalization. Its impact on the city remained that of a tiny brook in a city at flood tide.

The shift among activist women to political collaboration with men that Denison House and the WEIU demonstrate was not total but fragmented and strategic. It occurred early in the Progressive Era, not solely as a response to the failure of suasion, Freudian challenges, or a suffrage they did not yet have, or simply as a "domestication" of politics.⁸⁰ It was also a response to new opportunities. It bespoke an ironic socialization of women's groups—which sought greater power to transform society—into patterns, language, and aims more acceptable to the men who held power at the city's center and whose own mode of political behavior was also changing.

The result was more complex than cooptation of women or domestication of

⁷⁹ See, in particular, Mary Morton Kehew, president of the WEIU and the National WTUL, an active lobbyist at the state house and active in virtually every cross-class labor organization in Boston, as well as in Denison House and the Twentieth Century Club. According to Blair, *Clubwoman as Feminist*, 83, it was Kehew who brought Mary Kenney O'Sullivan to Boston to organize women workers. She set her up at Denison House, where Emily Green Balch was, later, the first head of the Boston WTUL. See Judith Becker Ranlett, "Sorority and Community: Women's Answer to a Changing Massachusetts, 1865–1895" (Ph.D. dissertation, Brandeis University, 1974), 180–81, and throughout, for such overlapping memberships.

It may well be that Kehew and others like her belonged to organizations with seemingly contradictory constituencies or philosophies precisely because of the different strategies, connections, and alliances each offered. The WEIU Annual Report for 1903–04, p. 11, notes that recent steps had been taken significant of a truly democratic policy "essential in its creed," an affiliation with the Union for Industrial Progress, which also had Kehew as its president, with the aim of organizing working women to improve their character and their condition; each organization would henceforth be represented on the board of the other, and, in addition (p. 12), it notes that they had organized a union of WEIU employees, in order to "become a real part of the Union for which they work."

⁸⁰ All of these are common explanations for the shift; see Ginzberg, *Woman and the Work of Benevolence*; Bordin, *Woman and Temperance*; Conway, *First Generation*; Baker, "Domestication of Politics."

men, for women did succeed in transforming not only themselves and the public roles of women but the city and its government somewhat, at least, in their own image. Within the middle-class world of gender, the city had learned to behave, because of these women, if not more like a sister and neighbor (the original vision), then more like a mother. The city now provided milk, school lunches, health clinics, vocational guidance, and kindergartens. Certainly, Denison House and the WEIU's vision in 1927 differed drastically from the one with which they had started, but so, too, was the city a different place. It may be that the nature of the welfare city that emerged from this era was influenced by the choices and compromises, the *realpolitik*, that these activist women confronted when trying to shape the city to their ends. This maternalist result had not inevitably arisen from women generalizing their experience of motherhood in "civic maternalism" or "municipal housekeeping." Rather, it was a statement about power relations (between men and women as well as between women), negotiations, and restricted choices.

AHR Forum

A New Intellectual History?

RUSSELL JACOBY

GENERALIZATIONS ABOUT SCHOLARLY DISCIPLINES require audacity. Abundant materials and findings of even small fields undermine, if not refute, generalizations. Who can keep up? In a single year (1987), scholars published 215 articles on John Milton, 132 on Henry James, and 554 on William Shakespeare.¹ "The sheer bulk of material being published," remarked Robert Markley, editor of the journal *The Eighteenth Century*, "means that it is almost physically impossible to read as fast as new readings are mass-produced."² Statements about "the" direction of Edmund Burke or Shakespeare studies inevitably seem misleading or wrong; several, perhaps scores, of specialists contradict a summary of the field.

Intellectual history is no different; generalizations about its direction or contours seem arbitrary. Although small, the field seems too large to permit valid statements about its direction. Is there a turn toward rhetorical studies? A revival of contextual approaches to great figures? A shift to popular ideas of social groups? These questions seem to sabotage clear answers. The matter always seems to depend on who is looking where.

For teachers, especially in graduate programs, at least one honorable reason justifies the slippery effort to reflect on and over a field: the need to educate others (and self).³ In addition, a unique reason spurs reflections on intellectual history: a nagging sense of decline.⁴ To prove a decay of intellectual history

¹ Edward B. Fiske, "Lessons," *New York Times* (August 2, 1989): B8.

² Robert Markley, "Stop the Presses: A Modest Proposal for Salvaging Literary Scholarship," *Eighteenth Century*, 29 (1988): 72. "It becomes increasingly difficult to decide what to read; confronted by the twenty-five entries in the 1984 *MLA Bibliography* under *Macbeth*, how can one decide which books and articles are worth reading?"; p. 72.

³ Another motivation surfaces: the desire to preserve or expand a professional field. Education passes into fief building: define a field so it (and its students) can survive and perhaps prosper. "I dislike to yield territory to sociologists, political scientists, etc.," wrote Frederick Jackson Turner, "on which the historian may raise good crops." Turner in a letter, September 5, 1914; cited by Michael Kammen, *Selvages and Biases: The Fabric of History in American Culture* (Ithaca, N.Y., 1987), 78. Dominick LaCapra admits that his own concerns about intellectual history partly bear witness to "an obvious 'territorial imperative.'" LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, 1983), 24.

⁴ "Decline from what?" or "From when?" it might be asked. Some date modern intellectual history from James Harvey Robinson's course at Columbia University before World War I; for instance, see Franklin L. Baumer, "Intellectual History and Its Problems," *Journal of Modern History*, 21 (1949): 191. Others look to Vernon L. Parrington's two volumes, *Main Currents in American Thought* (New York, 1927). Still others look to Arthur O. Lovejoy, his teachings, writings, and organizations. See Dorothy Stimson, "The History of Ideas Club," in George Boas, *et al.*, *Studies in Intellectual History* (Baltimore, Md., 1953), 174-96. In any event, John Higham, who has been monitoring its health for

convincingly is probably impossible—and unnecessary. It suffices to note that for some years social history attracted younger scholars and more attention. Intellectual historians were, or felt, neglected and excluded. Dominick LaCapra complained that intellectual history has been “relatively marginal to the discipline of history.”⁵ Referring to the “monumental bromide” that history has “many mansions,” he declared in the mid-1980s that “today social history tends to occupy many of the mansions and intellectual history a number of the shacks.”⁶ This perception—true or false—has inspired many articles about the “crisis” of intellectual history.⁷

Inasmuch as it forced intellectual historians to reconsider their identities—are they social historians *manqués* or something else?—the decline fostered an advance. “With a mixture of trepidation and excitement,” Martin Jay observes, intellectual historians have joined “the maelstrom of theoretical disputation that now characterizes the humanities as a whole.”⁸ John E. Toews remarks on “a new self-confidence” and a new common orientation among intellectual historians;

forty-odd years, reported that “as late as 1934,” an official bibliography of the American Historical Association ignored intellectual history. Higham, “The Rise of American Intellectual History,” *AHR*, 56 (April 1951): 462. Over the next decades, however, the cumulative impact of Charles Beard, Robinson, Parrington, Perry Miller, Merle Curti, Arthur Schlesinger, and others took effect. By the later 1940s, the quantity and quality increased to the point that some commentators believed intellectual history was “the profession’s outstanding achievement of the last decade.” Higham quoting Thomas Cochran in Higham, “Rise of American Intellectual History,” 467. In the 1950s, one historian referred to “the rapid rise of intellectual history to prominence in the world of American scholarship.” John C. Greene, “Objectives and Methods in Intellectual History,” *Mississippi Valley Historical Review*, 44 (1957–58): 58. Another noted it “has never been more popular. It attracts increasing numbers of students who regularly produce a sizeable body of very creditable work.” R. Richard Wohl, “Intellectual History: An Historian’s View,” *The Historian*, 16 (1953): 62.

⁵ Dominick LaCapra, *Soundings in Critical Theory* (Ithaca, N.Y., 1989), 199.

⁶ Dominick LaCapra, “On Grubbing in My Personal Archives: An Historiographical Exposé of Sorts,” *Boundary 2*, 13 (1985): 59.

⁷ Announcements of a “crisis” in the field started appearing in the late 1960s and intensified over the next fifteen years. By the early 1970s, Higham wrote of the field having “passed the zenith of its influence.” “American Historiography in the 1960’s,” in John Higham, *Writing American History: Essays on Modern Scholarship* (Bloomington, Ind., 1970), 169. A report from 1975 observed a “crisis” and malaise in that young scholars no longer specialize in intellectual history. “The field was no longer rising, but rapidly declining.” Gene Wise, “The Contemporary Crisis in Intellectual History Studies,” *Chio*, 5 (1975): 55. Intellectual history, offered another historian, “now seems as dated as narrow ties.” Paul K. Conkin, “Intellectual History: Past, Present, and Future,” in *The Future of History*, Charles F. Delzell, ed. (Nashville, Tenn., 1977), 111. Laurence Veysey’s survey of American historical scholarship in 1979 celebrated the productivity and diversity of American historians—and used the declining field of intellectual history to prove his point. “In 1976 some thirty-five to forty [book] titles alone were published in the rather arcane and unfashionable subfield of American intellectual history, an area that some scholars pronounce to be dying.” Veysey, “The United States,” in *International Handbook of Historical Studies*, Georg G. Iggers and Harold T. Parker, eds. (Westport, Conn., 1979), 157. A year after Veysey, Robert Darnton confirmed that disease had struck the field. “The trend toward self-doubt and beleaguered self-assertion can be found wherever intellectual historians discuss the state of their craft.” Once, intellectual historians, he observed, “saw their discipline as the queen of the historical sciences.” Now they have been dethroned and humbled. Darnton, “Intellectual and Cultural History,” in *The Past before Us: Contemporary Historical Writing in the United States*, Michael Kammen, ed. (Ithaca, N.Y., 1980), 327. The next year, William J. Bouwsma confirmed the diagnosis. “The decline of intellectual history appears obvious, and probably irreversible.” Bouwsma, “From History of Ideas to History of Meaning” (1981) in his *A Usable Past: Essays in European Cultural History* (Berkeley, Calif., 1990), 337.

⁸ Martin Jay, “The Textual Approach to Intellectual History,” in *Fact and Fiction: German History and Literature 1848–1924*, Gisela Brude-Firna and Karin J. MacHardy, eds. (Tübingen, 1990), 77.

they accept "semiological theory in which language is conceived of as a self-contained system of 'signs.'" ⁹ Michael Ermarth concurs. "There are now compelling reasons to speak definitely of 'old' and 'new' styles of European intellectual history." We are "in the midst of a shift in 'episteme.'" ¹⁰

Several historians have spearheaded the rethinking of intellectual history. Among these, perhaps the most committed is Hayden White. "No one writing in this country at the present time," LaCapra has stated, "has done more to wake historians from their dogmatic slumber than has Hayden White . . . One might, without undue hyperbole, say that White's writings have helped to reopen the possibility of thought in intellectual history." ¹¹ Allan Megill has judged White's *Metahistory* as "clearly the most important work" in its field "published in the last generation." ¹² A continuity marks White's work; he has always grappled with the venerable issue of the specificity of history. ¹³ His earlier contributions explored the relationship of history and science; he translated (or co-translated) two books on this topic, *From History to Sociology* (1959) by the Italian Crocean, Carlo Antoni, and *Human Sciences and Philosophy* (1969) by the French Marxist, Lucien Goldmann. Against the claims of the positive sciences, both books defended the uniqueness (and superiority) of history.

With chapters on Wilhelm Dilthey, Ernst Troeltsch, Friedrich Meinecke, and Max Weber, Antoni argued that German thought "declined" from historicism to sociology; invariant categories surrendered the flux and change of history. ¹⁴ Antoni faulted Dilthey for substituting a vacant typology in which "the types are always the same" for a "concept of becoming." Weber's sociology expressed "an epoch which has ceased to believe in history and has banished ideas of development, unfolding and progress to a place among the myths of optimism." Antoni concluded his discussion of Dilthey this way: "Once the real movement and novelty of history were denied, it was inevitable that history be transformed into typology and sociology . . . Dilthey in his uncertainty, oscillation and desperation

⁹ John E. Toews, "Intellectual History after the Linguistic Turn," *AHR*, 92 (October 1987): 881–82.

¹⁰ Michael Ermarth, "Mindful Matters: The Empire's New Codes and the Plight of Modern European Intellectual History," *Journal of Modern History*, 57 (1985): 507. For a very different perspective, see the spirited exchange between F. R. Ankersmit and Perez Zagorin. Ankersmit, "Historiography and Postmodernism," *History and Theory*, 28 (1989): 137–53; Zagorin, "Historiography and Postmodernism: Reconsiderations," and Ankersmit, "Reply to Professor Zagorin," both in *History and Theory*, 29 (1990): 263–96. See also the discussion in *Theory and Society*, in which Fritz Ringer argues for a more sociological intellectual history. Ringer, "The Intellectual Field, Intellectual History, and the Sociology of Knowledge," Charles Lemert, "The Habits of Intellectuals," Martin Jay, "Fieldwork and Theorizing in Intellectual History," and a "Rejoinder" by Ringer, all in *Theory and Society*, 19 (1990): 269–334. For a judicious overview, see Donald R. Kelley, "Horizons of Intellectual History: Retrospect, Circumspect, Prospect," *Journal of the History of Ideas*, 48 (1987): 143–69.

¹¹ LaCapra, *Rethinking Intellectual History*, 72.

¹² Allan Megill, Review Essay on Theodore Hamerow, *Reflections on History and Historians*, in *History and Theory*, 27 (1987): 98.

¹³ See Hans Kellner, "A Bedrock of Order: Hayden White's Linguistic Humanism," for a different view of White's development, in Kellner, *Language and Historical Representation: Getting the Story Crooked* (Madison, Wis., 1989), esp. 193–202.

¹⁴ For a good overview of Antoni, see Antonino Pagliaro, "Ricordo di Carlo Antoni," in Carlo Antoni, *Storicismo e antistoricismo* (Naples, 1964), 3–32. See also Gennaro Sasso, *L'illusione della dialettica: Profilo di Carlo Antoni* (Rome, 1982), esp. 39–72; and Michele Biscione, *Interpreti di Croce* (Naples, 1968), 95–187, which stresses Antoni's links to Benedetto Croce.

foreshadowed a crisis . . . In the throes of that crisis, the German intellectual world turned to the interpretation of history and life in a skeptical and relativistic mood."¹⁵

In a translator's introduction, White echoed these fairly desperate words as he outlined three forms of historicism. One dissolves history into the natural sciences; another unifies history and science by timeless metaphysical principles. In negating human responsibility and freedom, these historicisms were determinist and "vicious." However, a third form, what he called "aesthetic historicism," won White's sympathies; affirming "man's freedom and individual creativity," this historicism assumed that "a true vision of history must begin . . . with the subject, the historian living in the present." But it "went too far," White added, "abandoning reason completely."¹⁶ White's discussion remained vague, since individual historians went virtually unnamed. Dramatically (and elusively), he charged that all these historicisms expressed "partial views and thus unhealthy views, and this unhealthiness was manifested in the violence and anarchy that they spawned or justified."¹⁷

White's subsequent writings remain loyal to the fundamental ideas of Antoni or Goldmann.¹⁸ He defends historical knowledge as exceptional, and he resists subordinating history to science. Within this continuity, however, White's idiom and position have shifted. This can be put in two different ways; either he classifies history as a literary endeavor or he considers all the humanities essentially literary. He chips away at the uniqueness of history in order to accent its links to literature and literary criticism. This means that the stuff of literature—subjectivity, language, and rhetoric—is the stuff of history.

Two essays from the later 1960s, "The Burden of History" (1966) and "The Tasks of Intellectual History" (1969), might be regarded as White's prolegomenon to a new intellectual history; his tentative and earlier formulations became confident, even militant. In the first, he set forth the problem: for over a century, historians have defended themselves from scientists who derided their discipline as vague and subjective. The historians replied that history was not an exact

¹⁵ Carlo Antoni, *From History to Sociology: The Transition in German Historical Thinking*, Hayden V. White, trans. (Detroit, Mich., 1959), vii, 32, 167, 38.

¹⁶ White, "Translator's Introduction," Antoni, *From History to Sociology*, xix–xx, xxi–xxii.

¹⁷ White, "Translator's Introduction," xxiv. Of course, White may be referring to the knotty issue of historical thought and Nazism. To follow Georg Iggers, with Heidegger and others, "historicism . . . reached the end of its road: the last eternal values and meanings had dissolved. All that was left was historical, temporal, and relative." Iggers, *The German Conception of History* (Middletown, Conn., 1968), 244–45.

¹⁸ White wrote no introduction to the Goldmann text (Lucien Goldmann, *The Human Sciences and Philosophy*, Hayden V. White and Robert Anchor, trans. [London, 1969]), but he discussed Goldmann in an essay published the same year as his translation: Hayden White, "The Tasks of Intellectual History," *The Monist*, 53 (1969): esp. 623–25. In general, Goldmann, influenced by Georg Lukács and other Western Marxists, argued for the importance and meaning of history. "We are here concerned with a fundamental difference between history, which studies human behavior and the physico-chemical sciences, which study inanimate matter. The physico-chemical sciences study facts solely in their external or sensible aspect; the historian deals with consciously realized actions . . . of which he must, above all, discover the *meaning* . . . and an *objective* meaning which often differs from the conscious meaning in an important way." Goldmann, *Human Sciences and Philosophy*, 32–33. For a good overview of Goldmann, see his *Cultural Creation in Modern Society*, Bart Grahrl, trans. (Oxford, 1977), esp. the intro. by William Mayrl, 3–29; and Mary Evans, *Lucien Goldmann: An Introduction* (Sussex, 1981).

science like physics but “a kind of art.” Yet, when reproached by literary artists for failure to appropriate literary modes, the historians backtrack, claiming their field is a science and “historical data do not lend themselves to ‘free’ artistic manipulation.”¹⁹

For White, this position no longer satisfies. It irritates both scientists and artists, and it insulates historians from the latest artistic and scientific developments. In short, history’s mediating position between science and art is obsolete. The conclusion? Historians must embrace modern art and literature (and to some extent modern science). When historians defend the “art” of history, however, they usually envision nineteenth-century realism. They remain fixated on “antiquated notions” and “outmoded conceptions of objectivity.” Modern art has long surrendered the pretense of realism and objectivity. Historians must do the same.²⁰

This entails realizing that historical “explanation” is not exhausted by the “category of literally truthful” but can be judged by the “richness of the metaphors.” “Methodological and stylistic cosmopolitanism” necessitates understanding that “there is no such thing as a *single* correct view of any object under study, but that there are *many* correct views, each requiring its own style of representation.” Recognizing facts is “the problem that the historian, like the artist, has tried to solve in the choice of the metaphor.” While White alludes to the problem of “radical relativism,” he believes this program will allow historians to rejoin the artistic and intellectual dialogue of our time.²¹

“The Tasks of Intellectual History” signaled a more politically radical White dissatisfied with “the pessimistic and accommodationist tone of intellectual historiography.” Nietzschean and Marxist tones permeated his essay. “Intellectual history is rather like vicarious sex: neither satisfying nor, ultimately, very helpful as a guide to action . . . [I]ntellectual history substitutes for the color of the market place, the battlefield, and the parliament, the odor of the study, the library, and the academic hall.” He bemoaned the fact that the historical profession saddled intellectual history with conventional methods; when intellectual historians advance new perspectives, they must buck “*the essentially antitheoretical bias that prevails in the profession at large.*”²²

White’s *Metahistory* (1973) presented the ideas he had developed over the previous six years.²³ All history contains a “deep” verbal structure; this structure or metahistorical element shapes the histories. Exactly how White proceeds to analyze this structure is complicated and, at times, byzantine; for instance, he

¹⁹ Hayden White, “The Burden of History” (1966), in his *Tropics of Discourse: Essays in Cultural Criticism* (Baltimore, Md., 1978), 27.

²⁰ White, “Burden of History,” 29, 42–43. White discussed *Life against Death: The Psychoanalytic Meaning of History* (1959) by Norman O. Brown (then his colleague at the University of Rochester) as exemplifying a new artistic history. “By a series of brilliant and shocking juxtapositions, involutions, reductions, and distortions,” he “forces the reader to see with new clarity”; p. 45.

²¹ White, “Burden of History,” 47–48.

²² White, “Tasks of Intellectual History,” 608, 609–10, 616, italics in original.

²³ Richard T. Vann argues that Louis O. Mink’s work, for instance, his essay “History and Fiction as Modes of Comprehension” (1970), played a key role in White’s ideas; see Vann’s “Louis Mink’s Linguistic Turn,” *History and Theory*, 26 (1987): esp. 5–8. One element from White’s earlier program no longer appears in his later writings: the almost “new age” hope expressed in “The Burden of History” that history will embrace contemporary science as well as art.

finds sixty-four varieties of historical writing.²⁴ The main argument, however, is sufficiently clear. All history is inextricably poetic and linguistic; it interprets and molds facts, more than discovering or finding them. For this reason, histories can be approached as literature and analyzed using tropes of poetic language (metaphor, metonymy, synecdoche, and irony). This means—among other things—that no single history is more “realistic” than any other. “The best grounds for choosing one perspective on history rather than another are ultimately aesthetic or moral.”²⁵

In his essays from the mid-1970s, White elaborated on these ideas, championing a method underlining language, style, metaphor, and rhetorical strategies. “There has been a reluctance,” White wrote in “Historical Text as Literary Artifact” (1974) “to consider historical narratives as what they most manifestly are: verbal fictions, the contents of which are as much *invented* as *found* and the forms of which have more in common with their counterparts in literature than they have with those in the sciences.”²⁶

While White does not dismiss the “found” or factual dimension of history, he directs our attention to that formed or imagined by the historian. For instance, in a brief analysis, White argues that E. P. Thompson’s *Making of the English Working Class* is a contrived work; Thompson “makes” the English working class by consciously or unconsciously utilizing a basic tropic model in which “groups actually pass in a finite movement from a naive to an ironic condition in their evolution.” He has “lingered” on the “tropological unpacking of the structure of Thompson’s discourse,” White states, because Thompson “claims” to be studying “concrete historical reality.”²⁷

White’s contribution harbors many virtues; he forces historians to rethink their relationship to literature and style. “History as a discipline is in bad shape today,”

²⁴ White distinguished five levels in all historical work: chronicle, story, emplotment, argument, ideology. The first two involve how events are organized. The “plot” (or emplotment), however, is more decisive, since it is the manner in which the historian gives meaning to the chronicle/story. Following Northrop Frye, White suggests at least four different emplotments: romance, tragedy, comedy, and satire. “A given historian is forced to emplot the whole set of stories making up his narrative in one comprehensive or *archetypal* story form”; p. 8. Beyond the emplotment lies the explicit argument or the explanation, which are founded on “different notions of the nature of historical reality”; p. 13. White distinguished four types of historical explanation: formist, organicist, mechanistic, and contextualist. On the last level, the ideological, White also posited four types: anarchist, conservatism, radicalism, and liberalism. While there appear to be sixty-four possibilities—four different emplotments, explanations, and ideologies—White suggested elective and homological affinities exist among these levels; p. 29. In any event, White went on to state that a “particular *combination* of modes of emplotment, argument and ideological implication” makes up a “historiographical style.” This style is fundamentally linguistic and poetic, and was White’s real concern. It also can be classified in four ways, using the four basic literary tropes: metaphor, metonymy, synecdoche, and irony. In his essential book *In Defence of Rhetoric*, Brian Vickers judges severely White’s fetish of “four,” especially his reduction of rhetoric to four tropes. This he regards as of a piece with the “reduction, fragmentation, and misapplication of rhetoric in modern literary discourse.” Vickers, *In Defence of Rhetoric* (Oxford, 1989), 441–42, 453. Another historian of rhetoric, Nancy S. Struever, makes a similar and equally tough assessment of White; see her “Topics in History,” in *Metahistory: Six Critiques, History and Theory*, Beiheft 19 (1980): 66–79.

²⁵ Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, Md., 1973), xii.

²⁶ White, *Tropics of Discourse*, 82.

²⁷ Hayden White, Introduction: “Tropology, Discourse, and the Modes of Human Consciousness,” in White, *Tropics of Discourse*, 15–19.

he writes, "because it has lost sight of its origins in the literary imagination. In the interests of *appearing* scientific and objective, it has repressed and denied to itself its own greatest source of strength and renewal."²⁸ White insists that rhetorical choices saturate the historical project; all facts and accounts are already interpretations and decisions. His works compel historians to attend to their initial and fundamental preconceptions. This is all to the good.

However, his contribution also tilts toward relativism and, perhaps more serious, toward formalism. White almost bites the relativistic bullet. Historical material does not dictate anything; the historian selects and interprets. "Historical situations do not have built into them intrinsic meanings . . . Historical situations are not *inherently* tragic, comic or romantic . . . All the historian needs to do to transform a tragic into a comic situation is to shift his point of view . . . *How* a given historical situation is to be configured depends on the historian's subtlety in matching up a specific plot structure with the set of historical events that he wishes to endow with a meaning of a particular kind. This is essentially a literary, that is to say fiction-making, operation."²⁹

The problem is evident: insofar as intrinsic meaning is depreciated, the historian's discourse is elevated; the key is how he or she "emplots" material. The stuff of history can be plotted in numerous ways, dependent on skill. Does this mean that all histories are equally true and false? These objections are familiar to White. "I have never denied that knowledge of history, culture, and society was possible; I have only denied that a scientific knowledge, of the sort actually attained in the study of physical nature, was possible."³⁰ Or he writes that he acknowledges that "historical events" differ from "fictional events." What interests him are "the fictions of factual representation"—the way history and fiction "overlap, resemble or correspond with each other."³¹

Even as he seeks to avoid slipping into complete relativism, White comes exceedingly close. His essay "The Politics of Historical Interpretation" (1982) addresses the professionalization of history, which for White entails the "repression" of history's utopian, imaginary, and sublime moments. "Imagination is disciplined by its subordination to the rules of evidence." Conservatives, radicals, and Marxists rejected a "visionary politics" developed by Romantic and marginal thinkers—Jules Michelet, Friedrich Schiller, Friedrich Nietzsche—and by fascists. "We must guard against a sentimentalism that would lead us to write off such a conception of history simply because it has been associated with fascist ideologies. One must face the fact that when it comes to apprehending the historical record, there are no grounds to be found in the historical record itself for preferring one way of construing its meaning over another."³²

White digs a hole for himself and strains to climb out—unsuccessfully, accord-

²⁸ Hayden White, "The Historical Text as Literary Artifact," in *Tropics of Discourse*, 99.

²⁹ White, "Historical Text," *Tropics of Discourse*, 85.

³⁰ White, "Introduction," *Tropics of Discourse*, 23.

³¹ Hayden White, "The Fictions of Factual Representation," in *Tropics of Discourse*, 121.

³² Hayden White, "The Politics of Historical Interpretation," in White, *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, Md., 1987), 67, 72, 74–75. It is always tempting to turn the rhetoric of the rhetoricians against themselves. The critic of "facts" says here, "One must face the fact . . . there are no grounds." What fact?

ing to Carlo Ginzburg, who almost indicts White for succumbing to a Gentilean subjectivism and fascism.³³ Though politically the most explosive issue, relativism hardly exhausts the problems with White's *oeuvre*; moreover, it might divert attention from weaknesses more germane to intellectual history—White's formalism.

His formalism is explicit: White occasionally refers to himself as a "formalist." "My method, in short, is formalist."³⁴ The implications are not spelled out, however. "The tropological theory of discourse," states White, "could provide us with a way of classifying different kinds of discourses by reference to the linguistic modes that predominate in them rather than by reference to supposed 'contents' which are always identified differently by different interpreters."³⁵ The promise here is clear: abandon the "contents" (in quotes since they cannot really be established) for the linguistic structure. The problem is also clear: critical scrutiny and evaluation turn into categorizing. In looking at historical work, the questions become, how do we classify it? What are its principal tropes?³⁶

The relationship of categorizing to wider knowledge cannot be discussed in a brief compass—and probably no extended discussion suffices inasmuch as this relationship varies by the field of knowledge. For the humanities, categorizing tends to be insufficient, if not inadequate and superficial; it remains external, appraising structure and types. It smacks of a static, nonhistorical approach. It becomes formalistic.³⁷ White once knew this, since he criticized Arthur O. Lovejoy and Ernst Cassirer for this failing: "Their organizational principles are uniformly typological . . . [O]n the whole they avoided the problem of intellectual historical dynamics. They tended to view the history of consciousness as an intra-mural or domestic affair within consciousness itself."³⁸

Now White seems to do this himself. "A semiological approach to the study of texts," he writes, "permits us to moot the question of the text's reliability as witness to events or phenomena extrinsic to it, to pass over the question of the text's 'honesty,' its objectivity . . . This is to shift hermeneutic interest from the content of the texts being investigated to their formal properties."³⁹

³³ See Carlo Ginzburg's critique of White, "Just One Witness"; and commentary by Martin Jay, "Of Plots, Witnesses and Judgements: An Answer to Hayden White and Carlo Ginzburg" (unpublished papers). At least part of White's argument is neither wrong nor new; it has often been discussed that fascism and Nazism successfully exploited a mythic dimension that liberals, conventional conservatives, and Marxists abandoned. For instance, this argument has long marked the writings of George L. Mosse; see *The Nationalization of the Masses: Political Symbolism and Mass Movements in Germany from the Napoleonic Wars through the Third Reich* (New York, 1977). See also Jeffrey Herf, *Reactionary Modernism: Technology, Culture, and Politics in Weimar and the Third Reich* (1984; Cambridge, 1986), esp. chap. 1. Yet White seems to be stating something more than this—or perhaps less.

³⁴ White, *Metahistory*, 3.

³⁵ White, *Tropics of Discourse*, 21.

³⁶ For a critique of White's tropes for sidestepping the issue of historical truth, see Arnaldo Momigliano, "The Rhetoric of History and the History of Rhetoric: On Hayden White's Tropes," in *Comparative Criticism: A Yearbook*, 3, Elinor S. Shaffer, ed. (Cambridge, 1981), 259–68.

³⁷ This is a huge topic. There are innumerable versions of formalism: the formalism of Northrop Frye, New Criticism, and Russian Formalism—and many others. In politics and program, for instance, New Criticism and Russian Formalism seem very different. Nevertheless, they share certain elements. See Ewa M. Thompson, *Russian Formalism and Anglo-American New Criticism: A Comparative Study* (The Hague, 1971).

³⁸ White, "Tasks of Intellectual History," 613.

³⁹ White, *Content of the Form*, 192–93.

A fundamental irony colors White's formalism; his *oeuvre* develops out of a tradition underscoring the uniqueness of the humanities (and history). He consistently rejects as misguided efforts to imitate scientific neutrality and objectivity. Yet he bills his formalism as more objective, almost more scientific, than approaches that evaluate contents and contexts; indeed, he seems to dismiss these as impressionistic and subjective. The literary intellectual historian revels in the superiority of a hard-nosed formal and structural method. "The utility" of the semiological analysis, White proclaims, "is to be assessed solely in terms of quantitative criterion, namely, its capacity to account for more of the elements of any given text, of whatever length, than any contending 'content'-orientated method could match."⁴⁰ He offers an example of his method, an inspection of *The Education of Henry Adams*.

In this space, it is possible to comment on only one aspect of White's analysis: his own rhetoric. He assumes the idiom of the cool scientist scoffing at the emotionalism of the artist. He first dissects a 1961 preface to the book by D. W. Brogan, which seems to White completely "impressionistic and unsystematic." "It provides absolutely no criterion for assessing the validity of the various generalizations." Brogan's approach is completely unsuitable "as a model of analysis." It seems totally "arbitrary," based on "personal taste, inclination or ideological commitment." Against this intuitive and subjective commentary, White offers a "semiological perspective" that "can provide a theoretically grounded reading of this text, which would give an account for every element of it."⁴¹

Here, as elsewhere, White sounds like the analytic logician who can finally dispense with cloudy speculations of poet philosophers. The aim of a complete study of *The Education of Henry Adams* would be "to characterize the types of messages emitted in terms of the several codes in which they are cast and to map the relationships among the codes thus identified both as a hierarchy of codes and a sequence of their elaboration."⁴² White's introduction to *Metahistory* is titled "The Poetics of History." But this is the language—and cadence—of aggressive science.⁴³ White's rebellion against positivism ends in a scientific formalism.

WHITE HAS NOT BEEN ALONE in his effort to reorientate intellectual history; by his energy and productivity, Dominick LaCapra has joined and perhaps surpassed him. In the broadest terms, their contributions run parallel. They have both sought to rescue history (and intellectual historians) from a certain unconsciousness; they have forcefully drawn attention to the problems of language and text; they both want to push intellectual history in a literary direction.⁴⁴ Again, it is

⁴⁰ White, *Content of the Form*, 194.

⁴¹ White, *Content of the Form*, 196.

⁴² White, *Content of the Form*, 196–97, 208.

⁴³ In a sympathetic discussion, Reinhart Koselleck calls White's work a "scientifically grounded linguistic metahistory." Koselleck, "Einführung" to White, *Auch Klio dichtet, oder, Die Fiktion des Faktischen: Studien zur Topologie des historischen Diskurses* (Stuttgart, 1986), 6.

⁴⁴ See generally Lloyd S. Kramer, "Literature, Criticism, and Historical Imagination: The Literary Challenge of Hayden White and Dominick LaCapra," in *The New Cultural History*, Lynn Hunt, ed. (Berkeley, Calif., 1989), 97–128.

difficult to fault the project. Few can object to rethinking assumptions about intellectual history.

While they share an approach, LaCapra hardly follows White. Unlike White, he emphatically identifies with the field of intellectual history and worries about its health. Beginning with his second book, all LaCapra's works directly or indirectly broach questions of the methods and parameters of intellectual history. His book on Jean-Paul Sartre (1978) opens with a discussion of intellectual history.⁴⁵ His most recent collection (1989) closes with an essay, "Intellectual History and Critical Theory."⁴⁶

LaCapra is more pugnacious than White. "I have long felt that the annual convention of the American Historical Association is second only to that of morticians in the liveliness of its addresses and interchanges. A little controversy might . . . be welcomed."⁴⁷ LaCapra enjoys intellectual strife. He defends in a recent essay "the provocative role of hyperbole."⁴⁸ In addition, he boldly tackles subjects that seem especially unsuited for his method: LaCapra admits that Sartre has "explicitly rejected a theory of the text" that he employs.⁴⁹

He is also less systematic than White and, for that reason, is sometimes difficult to follow. This is not a statement about personal style but theoretical orientation. Despite their many similarities, White and LaCapra appeal to different traditions. Both reject a naïve, common-sense approach to history; both underscore the linguistic strategies of historical texts. Yet, in conceptualizing these strategies, White looks to the formalism of Northrop Frye, LaCapra to the deconstructionism of Jacques Derrida.⁵⁰ Like Frye, who always had an "obsession with form,"⁵¹ White prizes patterns that he believes structure texts, and, like Derrida, LaCapra challenges systems that he judges float above a text. Derrida's "oft-quoted maxim 'il n'y a pas de hors-texte' (there is no outside-the-text) is not a charter for formalism," LaCapra writes. "It is a critique of the attempt to ground the work and play of textuality in some extratextual foundation."⁵²

On this issue, LaCapra criticizes White. While unstinting in his praise, he also protests that White remains committed to the passive "historical record" and an "idealistic mythology." White fails to appreciate Derrida. For LaCapra, texts and language in White seem too traditional; White avoids the "problem of interplay

⁴⁵ The first words (of the main text) of his Sartre book run, "Intellectual history has often followed . . ."; Dominick LaCapra, *A Preface to Sartre* (Ithaca, N.Y., 1978), 19.

⁴⁶ Dominick LaCapra, "Intellectual History and Critical Theory," in *Soundings in Critical Theory*, 182–209.

⁴⁷ LaCapra, "On Grubbing in My Personal Archives," 46.

⁴⁸ LaCapra, *Soundings in Critical Theory*, 2.

⁴⁹ LaCapra, *Preface to Sartre*, 23.

⁵⁰ In keeping with his more rigorous formalism, White also refuses to distinguish between high and popular culture. "The linguistic model provides us with a basis for dissolving the distinction, hierarchial and essentialist in nature, between high culture on the one side and low, folk or popular culture on the other. This distinction . . . precludes the possibility of a genuine science of culture." Hayden White, "Structuralism and Popular Culture," *Journal of Popular Culture*, 7 (1974): 775. LaCapra resists losing all these differences, however; see, for instance, Dominick LaCapra, "Culture and Ideology," in LaCapra, *Soundings in Critical Theory*, 133–54.

⁵¹ A. C. Hamilton, *Northrop Frye: Anatomy of His Criticism* (Toronto, 1990), 40.

⁵² Dominick LaCapra, "Criticism Today," *Soundings in Critical Theory*, 19.

between structure and play in the text and one's relation to it"; he misses "the tense interplay among elements in the language of the text."⁵³

This is a persistent note in LaCapra, almost an anti-dogmatic dogma. Time and again, he insists on the complexities, ambiguities, and tensions of "the text." For instance, in *A Preface to Sartre*, he complains that "ambiguity is domesticated and controlled [in Sartre] by its insertion into the totalizing human project." LaCapra objects that "what is almost invariably left in the dark or repressed by Sartre is the intercourse between structure and play in his own use of language." His criticism of Sartre appeals to what LaCapra calls Derrida's "'minimal' program" that posits "the contest and contestation between structure and play." For this approach, Sartre's life can be ignored, since it offers few clues to his thought. "Sartre remains too much the same and does not change enough—at least as far as his writing is concerned."⁵⁴

LaCapra's next major effort examined the trial of Gustave Flaubert in 1857, a terrain in which "intellectual history and literary history converge." He explains that a trial "is a locus of social reading that brings out conventions of interpretation in a key institution." Indeed, he offers his book as a "test case" of his methodological program.⁵⁵ Is he successful? LaCapra's energy and devotion are admirable; his commitment to a close textual reading can hardly be faulted. For instance, he inspects the novel's dedication to Flaubert's lawyer, who defended the author when the government charged that *Madame Bovary* offended public morality; and LaCapra finds some extra punctuation in the translation. "Paul de Man, in his 'substantially new translation' of the novel, has introduced into the dedication six commas more than Flaubert himself used, thereby accentuating one's doubts about its intention."⁵⁶

Some of LaCapra's main points seem unexceptional. The trial "reduced the radical negativity of the novel to manageable proportions either to condemn or to praise its author."⁵⁷ For LaCapra, these juridical "readings" avoid the novel's seditiousness. Yet it is hardly surprising that neither the prosecutor nor defense went beyond a simple interpretation of *Madame Bovary*; neither were, or wanted to be, literary critics. They argued whether the book offended public morality by celebrating adultery. For LaCapra, this showed a superficial understanding of the novel.

LaCapra believes that *Madame Bovary* is marked by shifting narrative voices creating an "indeterminacy" that "unsettles the moral security of the reader." LaCapra has much to say about Flaubert's "dual style" and a variant of it, his "free indirect style," which also undermines a simple reading of the book. In fact, LaCapra has so much to say about Flaubert's style that, except in passing, he never returns to Flaubert's trial. Occasionally, he mentions that "the trial of course did not investigate the problem of the so-called free indirect style." In the conclusion,

⁵³ Dominick LaCapra, "A Poetics of Historiography: Hayden White's *Tropics of Discourse*," in *Rethinking Intellectual History*, 80–81.

⁵⁴ LaCapra, *Preface to Sartre*, 90, 26, 223, 39.

⁵⁵ Dominick LaCapra, "Madame Bovary" on Trial (Ithaca, N.Y., 1982), 7, 10.

⁵⁶ LaCapra, "Madame Bovary" on Trial, 53.

⁵⁷ LaCapra, "Madame Bovary" on Trial, 53, 54.

he reiterates that in understanding *Madame Bovary* neither the prosecutor nor defense "went beyond viewing the issue of standard conformity or deviance."⁵⁸

To be just a little unfair, the trial of Flaubert was for LaCapra a foil or excuse for a literary analysis of *Madame Bovary*. His learned and engaged discussion draws on numerous critics as well as Flaubert's letters and a few events of 1848. But LaCapra is not very interested in the trial or even in historically situating Flaubert's style. He analyzes Flaubert's style as found in *Madame Bovary*. "My own argument," he concludes, "has been that the problem of narration or modes of 'representation' in *Madame Bovary* is indeed complex . . . One minimal point I have tried to establish is that one cannot simply take the most lapidary statements concerning pure art . . . from the letters [of Flaubert] and interpret the novel as their unproblematic realization."⁵⁹ In his essays and books, LaCapra continuously repeats this "minimal point": texts cannot be reduced to other texts or contexts; texts are complex. As a method, this appears to be beyond dispute; no one wants to promote reductionism. Yet LaCapra's position becomes a dogma, a methodological pronouncement that sabotages interpretations.

LaCapra shares with other new intellectual historians several characteristics. They see themselves as beleaguered, a few rebels facing a powerful and unsympathetic profession. They generally stay clear of direct encounters with other historians; that is, they develop methodological critiques of historians but at arm's length. The method becomes an end in itself. In his major programmatic essay "Rethinking Intellectual History and Reading Texts," LaCapra reiterates that intellectual historians have succumbed to various reductionisms: a documentary approach, contextualism, presentism. "The predominance of a documentary approach," LaCapra writes, "is one crucial reason why complex texts—especially 'literary' ones—are either excluded from the relevant historical record or read in an extremely reduced way."⁶⁰ Yet he rarely tells us which historians or what histories are guilty of these sins.

This needs some qualification. The few efforts of new intellectual historians to criticize deficient history do not seem illuminating. The examples are usually secondary or marginal.⁶¹ Apart from briefly discussing E. P. Thompson, in one of his rare bids to demonstrate how "the figurative level of the discourse" guides a modern historian, Hayden White selects a passage from "a no-nonsense purveyor of facts." The passage he chooses comes from A. J. P. Taylor's *Course of German History*.⁶² This is an odd choice. Taylor's book is generally considered extremely tendentious and polemical; few books are less neutral. All the details are marshaled to show that Luther led to Hitler. It takes no great acumen to show that Taylor's language abets his project.

To his credit, LaCapra has sought several direct encounters with writings of other historians. Yet the results are decidedly mixed. Against provocative interpretations, LaCapra argues that texts are complex and indeterminate. He

⁵⁸ LaCapra, "Madame Bovary" on Trial, 60, 127, 210.

⁵⁹ LaCapra, "Madame Bovary" on Trial, 211.

⁶⁰ LaCapra, *Rethinking Intellectual History*, 33.

⁶¹ See Anthony Pagden's comments in "Rethinking the Linguistic Turn: Current Anxieties in Intellectual History," *Journal of the History of Ideas*, 49 (1988): 527.

⁶² White, *Tropics of Discourse*, 107–08.

evaluates Wittgenstein's *Vienna* (by Allan Janik and Stephen Toulmin), which he states belongs to "the standard repertory of the intellectual historian." The book puts forth "a striking argument" based on a "contextualist approach" that interprets a text by way of other writings and individuals.⁶³ LaCapra finds "extremely reductive" their argument that the *Tractatus logico-philosophicus* is fundamentally ethical. "The text becomes a vehicle for ideas, discursive arguments, and essential positions." He objects to their appeal to Karl Kraus, Robert Musil, Wittgenstein's letters, and *fin-de-siècle* Vienna to bolster their case. Yet LaCapra does not go much beyond arguing that Wittgenstein and the *Tractatus* preclude any interpretation. It is fair enough to question Wittgenstein's emphatic statement about the ethical core of the *Tractatus*, but LaCapra seems to prefer ambivalence and indeterminacy.

Does the relation between what Wittgenstein directly and indirectly states, LaCapra asks, "authorize one to say that the text is essentially ethical—or essentially anything else for that matter?" In effect, LaCapra answers negatively; the text is essentially about nothing (or is essentially nothing). Wittgenstein's *Tractatus*, like any text, is "the scene of an interplay between different forces—forces of unification and dissemination—whose relation must be taken as a matter of inquiry."⁶⁴ To LaCapra, a convention of historians resembled an assembly of morticians, but here one sniffs the stale air of endless seminars. Nothing is ever figured out or resolved; everything is postponed to next week's meeting.

His critique of Carlo Ginzburg's *Cheese and the Worms* raises valid objections yet returns insistently to the proposition of textual complexity. "My emphasis would be on the complex, often distorted interaction of levels or aspects of culture and the attendant relations between orthodoxy and heterodoxy in social and intellectual life."⁶⁵ Ironically, the critique of Ginzburg by this militantly unconventional or anti-traditional historian could have been made by an ordinary empiricist who prizes facts. LaCapra charges that Ginzburg fails to sift carefully through his texts, generalizes too widely and boldly, is too anecdotal and informal, and does not offer enough evidence. He complains that "the diffuse narrative and anecdotal style [of Ginzburg] facilitates the treatment of it [oral culture] in vague, piecemeal, and allusive terms."⁶⁶

The point is not that LaCapra is wrong but that, in one of the few detailed critiques of other historians, his own position is little more than a call for textual prudence.⁶⁷ Unhappy with Ginzburg's interpretation of an inquisition report, he

⁶³ All citations from Dominick LaCapra, "Reading Exemplars: Wittgenstein's *Vienna* and Wittgenstein's *Tractatus*," in LaCapra, *Rethinking Intellectual History*, 84–117.

⁶⁴ LaCapra, "Reading Exemplars," 102, 116.

⁶⁵ Dominick LaCapra, "*The Cheese and the Worms: The Cosmos of a Twentieth-Century Historian*," in his *History and Criticism* (Ithaca, N.Y., 1985), 63.

⁶⁶ LaCapra, "*The Cheese and the Worms: The Cosmos of a Twentieth-Century Historian*," 55.

⁶⁷ LaCapra has also discussed the work of Carl Schorske, Robert Darnton, and Roger Chartier. See his "Is Everybody a *Mentalité* Case? Transference and the 'Culture' Concept," in his *History and Criticism*; and "Chartier, Darnton and the Great Symbol Massacre," in his *Soundings in Critical Theory*. Again, LaCapra makes some good points, but he repeatedly objects to the reductionist "reading" of texts. "One question that nonetheless rises as Schorske carries out his program," he asks, is whether he "tends to collapse in a reductive direction whereby the artifact or text is explained as a very restricted . . . response to a 'larger context.'" *History and Criticism*, 82. "On a methodological level, an overly reductive process is operative in Darnton's own understanding of reading and symbolic

complains, "At the very least, the reader deserves a transcription of the inquisition register itself to be in a better position to test the use and the interpretation made of it."⁶⁸ LaCapra sounds very much like a traditionalist upset that a researcher has been insufficiently cautious.

As with White, what begins as a call for a radical departure in historical thought closes sounding very familiar: the importance, complexity, and ambiguities of the text are continuously reiterated—a proposition advanced for decades by new and not-so-new literary critics. To be sure, what is familiar is neither bad nor wrong. Yet, as with White, in two respects the circle has been closed. The new intellectual historians undercut emphatic arguments with the truism of textual complexity. The world of interpretations turns gray; everything is complicated, indeterminate, feasible. Moreover, the method becomes the object. LaCapra has criticized the fetish of archives and documents, as if facts themselves advance knowledge and truth; however, he has glorified a textual technique, as if method exists apart from its object.

Might White and LaCapra despite themselves harbor affinities toward a positivist science? The point is not to fling about vague charges over some alleged scientism, as if science were evil. Rather, it is this: White, LaCapra, and other new intellectual historians emerge out of a tradition that sought to rescue, if not cultivate, the uniqueness of history; this project does not entail denigrating science but distinguishing history from its formal, quantifying, and objective elements. Yet the very language of the new intellectual historians (and often the program) smacks of a formalism and objectivity associated with the sciences.

The theorists of a literary intellectual history disdain the "impressionist and unsystematic," White's objection to D. W. Brogan's preface. They want the language of rigor, structure, and technique. Often, LaCapra criticizes a historian's language; he never states that he finds it too literary, but his objections amount to the same. For LaCapra, the idiom seems too pictorial or accessible or subjective. These are defects, which LaCapra implicitly contrasts to language that is abstract, technical, structured—the vocabulary of the professional scholar.

LaCapra criticizes the "premium" bestowed on "straightforward prose (the no-jargon rule)."⁶⁹ He faults "the diffuse narrative and anecdotal style" of Ginzburg's *Cheese and the Worms*.⁷⁰ He protests that Darnton is "too accommodatingly readable," and this style lends itself "to gloss over problems and smooth over knotty points that may call for critical thought."⁷¹ He objects to the conformity of the traditional "men and ideas" intellectual history with its "highly readable but diverting mode of introducing readers to the text."⁷² He finds that Carl Schorske's

meaning." *Soundings in Critical Theory*, 79. LaCapra is responding to Darnton's book, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1984), as well as a discussion of it by Darnton and Chartier; the latter's "Text, Symbols and Frenchness," is now in his collection, Roger Chartier, *Cultural History: Between Practices and Representations*, Lydia G. Cochrane, trans. (Cambridge, 1988), 95–111.

⁶⁸ LaCapra, "The Cheese and the Worms: The Cosmos of a Twentieth-Century Historian," 63.

⁶⁹ LaCapra, "Intellectual History and Critical Theory," 196.

⁷⁰ LaCapra, "The Cheese and the Worms: The Cosmos of a Twentieth-Century Historian," 54–55.

⁷¹ LaCapra, "Chartier, Darnton and the Great Symbol Massacre," 82.

⁷² LaCapra, *Preface to Sartre*, 20.

polished style mirrors his simplified vision of Viennese culture. With Schorske, "an almost Viennese flair for the elaborate elegance of the nicely turned phrase and a butterflylike delicacy in moving from *topos* to *topos* . . . engender an enchanting world of words."⁷³ The palpable disdain for the less than technical prose suggests the new intellectual history secretly admires the science it openly rejects.⁷⁴

OTHER HISTORIANS HAVE FOLLOWED OR ACCOMPANIED White and LaCapra in advancing a new intellectual history; unfortunately, their contributions—for instance, Sande Cohen's *Historical Culture*,⁷⁵ Allan Megill's "Recounting the Past: 'Description,' Explanation, and Narrative in Historiography,"⁷⁶ and David Harlan's "Intellectual History and the Return of Literature"⁷⁷—reveal the same ills. These pieces share an extravagant appeal to new theories and approaches, a dramatic portrayal of the depth and power of the opposition to these theories coupled with no clear reference as to who or what constitutes that opposition, and a sketch of a new intellectual history that outlines a new formalism or textual prudence.

Cohen's *Historical Culture*, however, may be *sui generis*. By reason of its dense jargon, *Historical Culture* resists not simply a comparison with other new intellectual histories but any evaluation. Yet it features an enthusiastic blurb by White. "The most original contribution to historiographical theory since Paul Ricoeur's *Time and Narrative* . . . A brilliant achievement!" LaCapra also reviewed it generously. "Cohen is on the mark in mounting a sustained critique of the recent overvaluation of narrative, and his plea for theory and criticism is timely and forceful."⁷⁸ And Cohen's main argument parallels that of the other new intellectual historians.

Liberal citing Roland Barthes, Jean Baudrillard, Jean-François Lyotard, and postmodern French critics, Cohen identifies his work as a "modified version of deconstruction theory." Like other new intellectual historians, he challenges an oppressive order, "academic historiography" that is "part of the overall requirement for cultural stability."⁷⁹ His method—if it can be called that—is formalistic inasmuch as it dissects the language of historians.

Cohen's own language requires more than a passing comment. His prose is so opaque that the book comes with a glossary, but unfortunately the glossary needs a translator or an editor. "Actantial/actant" reads an entry: "refers to the complex exchange between what a 'historical' narration allows to be the subject of doing

⁷³ LaCapra, "Is Everyone a *Mentalité* Case?" 84.

⁷⁴ To be fair, LaCapra has raised the problem of "expertise . . . becoming enclosed in its own dialect or jargon"; see "Rethinking Intellectual History and Reading Texts," in his *Rethinking Intellectual History*, 65.

⁷⁵ Sande Cohen, *Historical Culture: On the Recoding of an Academic Discipline* (Berkeley, Calif., 1986).

⁷⁶ Allan Megill, "Recounting the Past: 'Description,' Explanation, and Narrative in Historiography," *AHR*, 94 (June 1989): 627–53.

⁷⁷ David Harlan, "Intellectual History and the Return of Literature," *AHR*, 94 (June 1989): 581–609.

⁷⁸ Dominick LaCapra, in *AHR*, 92 (April 1987): 376. LaCapra also made some serious criticism.

⁷⁹ Cohen, *Historical Culture*, 10, 17.

(for example, capitalism treated as the actant of innovation or capitalism presented as the subject of dialectical transformations) and the reader's ability (generally) to acknowledge primary roles of action as necessary to a culture."⁸⁰ This is Cohen when succinct.

Cohen admits that his terms are "radically unfamiliar," but it "must be so." The usual academic discourse evades and absorbs. His "decoding" requires "a discourse that slows reading, that refuses to convince a reader by its cadence or even rightness." He succeeds in this. "If transcendence is a permanent possibility of semiotic-intellectual destruction," runs a typical sentence, "because its minimal function is to make unthinkable the negation of that 'which ties one to reality' and holds one in place, this superfunction today is perfected in contexts where language is hypervalorized as the 'indispensable,' 'needed,' 'necessary,' 'required,' and so on, basis of enculturation."⁸¹ The question is less Cohen, however; it is whether the concentration on language and texts by the new intellectual historians ignores language and texts—their own. Does their critique of narrative foster the illusion that conceptual subversion requires an unreadable prose? This seems to be the case. Again, the paradox is striking. These historians seek to restore or rethink history's links to the humanities and literature. They want to free history of scientific pretensions; they ponder metaphors, rhetoric, and imagination. Yet they author, even champion, insular and cramped writings distant from literature.

A rare devotion to an argument distinguishes *Historical Culture*. Cohen claws his way through historical texts (by Peter Gay, E. P. Thompson, and Fernand Braudel) sentence by sentence for hundreds of pages. This is no mean achievement; such intensity is typically brought to bear on poetry, short fiction, or the densest philosophy, not the prose of historians. Cohen cannot be charged with avoiding specifics. By virtue of its singlemindedness, *Historical Culture* makes a point. One example must suffice for Cohen's method. He spends well over a page on the first sentence of Peter Gay's appendix to *Weimar Culture*, which reads "The Weimar Republic was proclaimed on November 9, 1918, by the Social Democrat Philipp Scheidemann."⁸² Cohen objects to the pretense of neutrality and matter-of-factness: Gay writes neither "As I hope to prove, the Weimar Republic was proclaimed in order to . . ." nor "what was *called* the Weimar Republic by x." Rather, Gay's sentence encourages "passive cognition." "The reader is blocked from considering the status of such namings." The opening sentence, Cohen

⁸⁰ Cohen, *Historical Culture*, 327.

⁸¹ Cohen, *Historical Culture*, 2, 46. "No historian who reads and comprehends this book will ever write in the same way again." Is this blurb of Cohen's book by Professor Mark Poster praise or condemnation?

⁸² Peter Gay, *Weimar Culture: The Outsider as Insider* (New York, 1968), 147. To be sure, Cohen's choice of writings seems somewhat eccentric, if not misleading. For instance, when he takes up Peter Gay, he does not select Gay's *Freud* or *Voltaire's Politics* or *Bourgeois Experience* but what he calls a text "representative of historical works read by an audience as a confirmation of the act of synthesis"; p. 110. An inattentive reader might think he is analyzing the conclusion to Gay's *Weimar Culture*, but it is an appendix to that book. Gay directs those "unfamiliar with modern German history" to this appendix but observes that it "obviously makes no claim to originality"; Gay, xv. Cohen employs sixty pages to reflect on this seventeen-page appendix, but he never manages to state the obvious: this text was a minor addition to the main book.

concludes elegantly, is “both a performative (see Barthes) and what Derrida has called a detour for the reappropriation of presence.”⁸³

The argument complements that of other new intellectual historians and its truth remains salutary. Narratives suppress issues by carrying along the reader. Even the plainest sentences betray cultural assumptions; and insofar as historians write—or study texts—they must weigh language. This radical insight, however, slides into its opposite: a new cautiousness. Nothing can be stated without multiple qualifications and clauses. Sprinkles of “as I hope to show” and “what is called” or scare quotes around every other word supposedly subvert a passive narrative. LaCapra also backed away from decisive arguments of Ginzburg, Toulmin, and Janik. “As I see it,” Cohen writes in his conclusion citing Baudrillard, the historical narratives exclude the “reciprocity and antagonism of interlocutors, in the ambivalence of their exchange.” They leave out the questions “‘What do you mean by . . . ?’”⁸⁴ These questions are essential, but an obsession with ambivalence and interchanges hardly better an obsession with clear facts.

David Harlan also wants to restore and rethink the link between history and literature. He writes that “now, after a hundred-year absence, literature has returned to history . . . The return of literature has plunged historical studies into an extended epistemological crisis.” Apart from stating that the “return of literature” was “prepared” by Ferdinand de Saussure, Harlan makes no effort to indicate when literature deserted history. What is this “one-hundred year absence”?⁸⁵ Does it include, for instance, Vernon Parrington’s *Main Currents in American Thought*? In his *American Intellectual Histories and Historians*, Robert Allen Skotheim calls this “no doubt the most famous American history of ideas ever published” and notes that it was frequently criticized for excessively discussing literature.⁸⁶ Or, if this is too far afield, what of Schorske’s *Fin-de-siècle Vienna* (1980)? Its first chapter is titled “Politics and Psyche: Schnitzler and Hofmannsthal.”⁸⁷ Or James H. Billington’s *Icon and the Axe* (1966)?⁸⁸ Or Raymond Williams’ *Culture and Society 1780–1950* (1958)?⁸⁹ Do these belong to the period when intellectual historians expelled literature? To Harlan, probably none of these count, since they may be too flatfooted in their employment of literature. Harlan is not really bothered by absence of literature from history, however; he is irked by the absence of a different beast: literary theory.

This sleight-of-hand or confusion facilitates the stance that Harlan shares with

⁸³ Cohen, *Historical Culture*, 113. To be sure, this sentence includes references to the relevant pages in Barthes and Derrida, for instance, “(see Barthes 1970: 145).” Unfortunately, nothing resembling “performative” appears on p. 145 of the cited work. Barthes, “Historical Discourse,” in *Introduction to Structuralism*, Michael Lane, ed. (New York, 1970).

⁸⁴ Cohen, *Historical Culture*, 325.

⁸⁵ “For a provocative discussion of literature’s return specifically to history,” Harlan refers the reader to Linda Orr, “The Revenge of Literature: A History of History,” *New Literary History*, 18 (Autumn 1986): 1–22. Yet this piece contains only the most marginal and fleeting references to historians. Harlan, “Intellectual History and the Return of Literature,” 581 n. 1.

⁸⁶ Robert Allen Skotheim, *American Intellectual Histories and Historians* (Princeton, N.J., 1966), 145–48.

⁸⁷ Carl E. Schorske, *Fin-de-siècle Vienna: Politics and Culture* (New York, 1981).

⁸⁸ James H. Billington, *The Icon and the Axe: An Interpretive History of Russian Culture* (New York, 1966, 1970).

⁸⁹ Raymond Williams, *Culture and Society, 1780–1950* (Garden City, N.Y., 1958, 1960).

other new intellectual historians; he also sees himself as besieged, combating a serious and well-armed enemy. He calls the opposition (the "contextualists"), a powerful force, "the dominant and now conventional orthodoxy . . . well placed, well organized and increasingly intolerant of alternative approaches." He offers as evidence John P. Diggins' book on Thorstein Veblen, *The Bard of Savagery*. Diggins' effort to "recontextualize" and "rewrite" Veblen received a critical review and comment in several historical journals, meaning "the American historical establishment would have none of it."⁹⁰

Harlan also alludes to one of the odder pieces by LaCapra, "On Grubbing in My Personal Archives." Although LaCapra also challenges contextual reductionism, he made an exception. LaCapra found highly significant that his own book *"Madame Bovary" on Trial*, which he characterizes as a book by "a full professor at a 'major research institution,'" was sharply criticized in the "official publication of the American Historical Association" by an assistant professor from the bush leagues. Moreover, after a reply from LaCapra, James Smith Allen of Phillips University in Enid, Oklahoma, did not back down.⁹¹ LaCapra took umbrage.

I would suggest that the very fact that a relatively unknown assistant professor from a relatively unknown university is willing not only to write a critical review but to follow it up with a rather imperious letter [a reply to LaCapra's response] indicates that he must be fairly certain he is indeed invoking conventional wisdom and will have the large majority of the profession in his corner . . . [T]he hegemonic voice of historiography . . . speaks through him [the reviewer].⁹²

In Allan Megill's essay, there is less posturing, yet he, too, vaguely refers to an army of historians stamping out new theories. Opponents are everywhere, but he hardly finds any. His examples of historical writings also seem askew: only one comes from a historian, another he invents, still others derive from textbooks. He shares with Harlan an ostentatious appeal to sophisticated paradigms, hinting that common historians will be unable to follow his subtlety. "The force and implications of this essay's distinction between recounting and explanation," Megill notes toward his conclusion, "are likely to be misunderstood by many readers." Or he states, "Yet, even among historians of some sophistication, there remains a tendency to underrate the force and scope of the hermeneutical insight that all perception is perspectival."⁹³

What are the issues? The idioms of Harlan and Megill diverge from one

⁹⁰ Harlan, "Intellectual History and the Return of Literature," 594, 607. For some reflections on Harlan's essay, see Joyce Appleby, "One Good Turn Deserves Another: Moving beyond the Linguistic," *AHR*, 94 (December 1989): 1326-32.

⁹¹ See *AHR*, 88 (June 1983): 805-07. Allen is author of numerous essays on nineteenth-century French reading and publishing, and two books: *Popular French Romanticism: Authors, Readers and Books in the 19th Century* (Syracuse, N.Y., 1981); *In the Public Eye: A History of Reading in Modern France, 1800-1940* (Princeton, N.J., 1991).

⁹² LaCapra, "On Grubbing in My Personal Archives," 49.

⁹³ Megill, "Recounting the Past," 646, 636. Megill likes formulations suggesting that even the most sophisticated fall behind Megill. In its pedantic one-upmanship, the following satirizes itself: "Even historians aware of the hermeneutic tradition often resist the self-reflexive implications. Note, for example, Quentin Skinner's apparently unwitting reduction of post- to pre-Heideggerian hermeneutics"; p. 637 n. 38. Since text is the name of the game, it is tempting to begin at the beginning of his text. Megill thanks four research assistants for their help and thirty colleagues for their comments. For an essay, this is an act of intimidation.

another—in fact, they are almost opposite—but their general efforts run parallel. Like LaCapra and White, they resist what they consider scientistic approaches to history and texts, and they appeal to new literary theories. Harlan argues against both the “radical contextualists” and those who seek “to recover authorial intention.” Postmodern literary theory subverts these reductionist methods. “Texts do not point backward, to the historical context or putative intentions of their now-dead authors; they point forward, to the hidden possibilities of the present.”⁹⁴

Harlan becomes a bit misty-eyed and mystifying; he writes about the need to recognize that “every text, at the very moment of its inception, has already been cast onto the waters, that no text can ever hope to rejoin its father, that it is the fate of every text to take up the wanderings of a prodigal son that does not return.”⁹⁵ Megill is more sober. He challenges “professional orthodoxy” that elevates causal explanation over more subjective interpretations or descriptions.⁹⁶ For two reasons, historians still denigrate descriptions and esteem scientific approaches and explanations: their “prejudice for universality” and their “hermeneutic naïveté, or the belief in immaculate perception.” Both are derived from the continuing prestige of “science,” especially the belief that science requires a neutral observer seeking general laws. “It is widely held in philosophy and in social science that only knowledge of the general or universal (as distinguished from the local or particular) is truly scientific.”⁹⁷

Turning first to *The Mediterranean and the Mediterranean World*, Megill challenges conventional opinion by avowing that Braudel’s masterpiece is “narrative history.” This truth has been missed because of a traditionalism that identifies narrative as a chronicle or sequence of actions; yet narrative is much more, combining four elements: action, happening, character, and setting. The first two are “events,” which means they “occur”; the latter two are “existents,” which means they

⁹⁴ Harlan, “Intellectual History and the Return of Literature,” 604. See the criticism of Harlan following his article: David A. Hollinger, “The Return of the Prodigal,” *AHR*, 94 (June 1989): 610–21.

⁹⁵ Harlan, “Intellectual History and the Return of Literature,” 600.

⁹⁶ Megill, “Recounting the Past,” 631.

⁹⁷ Megill, “Recounting the Past,” 632, 634. Megill’s essay may be bidding not simply for a shift toward a hermeneutical history but for itself as a notable effort on that behalf; rhetorically, it almost offers itself as a rejoinder to Carl G. Hempel’s 1942 “Function of General Laws in History” (which Megill cites), which was required reading for a positivist philosophy of history. Hempel sought to push history toward the exact sciences by promoting a search for general laws. His essay began: “It is a rather widely held opinion that history . . . is concerned with the description of particular events of the past rather than with the search for general laws.” Hempel considered this “unacceptable” and tried to show why. Hempel, “The Function of General Laws in History,” in Patrick L. Gardiner, ed., *Theories of History* (Glencoe, Ill., 1959), 344–45. Forty-five years later, Megill begins almost identically but argues the reverse proposition: “It is a rather widely held opinion among professional historians that the truly serious task of historiography . . . is the task of explanation,” that is, causal and general laws. It is difficult to figure out why Megill imagines that in the decades since Hempel’s essay the positivist currents have become stronger; Hempel and others who advanced similar arguments did not make much impact, and surely their influence has diminished, not increased, since the 1940s. “When Carl Hempel applied Karl Popper’s concept of a ‘covering law’ to history,” James T. Kloppenberg has written, “and when Mandelbaum elaborated his own objectivist theory, few historians were persuaded.” Kloppenberg, “Objectivity and Historicism: A Century of American Historical Writing,” *AHR*, 94 (October 1989): 1022.

“simply are.” “Emphasis on one of the four elements limits the attention given to the others.” To clarify, Megill offers a formula:

$$(AH) \times (CS) = k$$

which means “action times happening [that is, ‘events’] times character times setting [that is, ‘existents’] equals a constant.” Unfortunately, what these elements are, and why they must stand in an inverse relation, remains obscure; we are assured, however, that only traditionalists or the “uninformed” will deny the truth of this formula.⁹⁸ With this formula, we see that Braudel’s book is narrative history.

The same irony that marks the work of White and LaCapra is evident here. The new intellectual historians resolutely seek to escape from a baneful positivism that erases the specificity of history; they reject a search for general, causal, and objective laws; they want to attend fully to the ambiguities of the text and the subjectivity of the historians who interpret the texts. These laudable aims, however, vaporize in the course of their contributions. Instead of reviving historical thinking and nurturing subjectivity, they promote empty taxonomies and scientific idioms. They celebrate bold theories and revel in cautious truisms. They cherish a more literary history and offer pale methodologies and systems.

New history, new social history, new intellectual history: especially in history, “new” is suspect. But that does not mean nothing changes or should not change. By their energy and thoughtfulness, the new intellectual historians have brought a welcome ferment to a quiet field; and the story is far from over. An initial report suggests, however, that they succumb to bloodless scholasticism and cold formalism.

⁹⁸ Megill, “Recounting the Past,” 645.

AHR Forum
Intellectual History and Its Ways

DOMINICK LACAPRA

THERE IS MUCH IN RUSSELL JACOBY'S WORK that I respect and admire, especially his attempt to keep alive the tradition of critical theory and the role of the public intellectual. But there are aspects of "A New Intellectual History?" that I find questionable both in relation to my own work and to broader issues in the field.

It might be useful to begin with an adaptation of a question raised by Jacques Lacan in the wake of Sigmund Freud: What does Jacoby want? From "A New Intellectual History?" it is difficult to answer this question. The essay recognizes some virtues in the "new" intellectual history, but whatever is good in the new is really old. Whatever is more or less new tends to be bad, except of course for the begrudging qualification introduced by a conventional bow to the inevitability of change and the value of ferment. In the intriguing role of a populist increasingly conservative Theodor Adorno who has his doubts about newfangled ways, Jacoby elaborates a common-sense negative dialectics that merges with the good old genre of the jeremiad. (Lost in the process is Adorno's own sharp sense of the possibly necessary and desirable nature of difficulty and experimentalism.) For Jacoby, things are bad and they seem to be getting worse. Skittish colleagues are running after novelties and turning virtue into vapor. What Jacoby would himself propose as a constructive alternative is left to the reader's imagination. Thus he concludes on a note that mitigates resolute castigation with a token gesture in the direction of suspended judgment:

New history, new social history, new intellectual history: especially in history, "new" is suspect. But that does not mean nothing changes or should not change. By their energy and thoughtfulness, the new intellectual historians have brought a welcome ferment to a quiet field; and the story is far from over. An initial report suggests, however, that they succumb to bloodless scholasticism and cold formalism.¹

This ending rings a little false. Jacoby's is hardly an "initial report." His own footnotes indicate that he is following a long line of historians who have pondered, pranced, pouted, and fumed over "newer" tendencies in the field. This is not to say that the report is all wrong, but it is neither initial nor very novel. One self-contradictory feature of Jacoby's essay is his inclination to disparage claims of

¹ Russell Jacoby, "A New Intellectual History?" *AHR*, 97 (April 1992): 424.

the “new intellectual historians” that they are a beleaguered lot and then to join in beleaguering them in very familiar tones indeed. So what else is new?

I shall refrain from commenting directly on Jacoby’s views of other so-called new intellectual historians. I hesitate to preempt others’ responses or to propose myself as the representative voice of a prefabricated group or movement. Instead, I shall focus on Jacoby’s understanding of what I have been trying to do, enter into what I trust will be a critical but constructive exchange with him, and conclude with an indication of my current views about the field. I hope my remarks will have a broader bearing on debates about how to do intellectual history.

With respect to those he includes under the rubric “new intellectual history,” Jacoby notes some differences, but he is clearly most concerned with what is shared, particularly such things as bloodless scholasticism and cold formalism. With respect to my own work, I am surprised by the charge of formalism, since this is what I myself tend to criticize and to shy away from, perhaps to a fault. For I in fact think that a concern with form is important both in what we study and in how we study it, but form should not be separated out and reified in a methodology or constituted as an object of exclusive interest. If by scholasticism Jacoby means a concern with careful and at times intricate argument or with professional competence and specialized knowledge, I plead guilty, but his own comments would indicate that my scholasticism is often impassioned and even combative. For me, ideas are not cold and bloodless but insistent, important, and even erotic things.

What the preceding comments should serve to bring out is that Jacoby is himself largely a lumper in his attempt to locate a school of thought and to provide the essential characteristics of its thinking. This lumping tendency is important for Jacoby’s approach to problems. It is crucial for characterizing the work of others in simple, readily readable prose immediately accessible to a large public. His decided penchant for clear-cut characterizations and categories also helps to explain what he focuses on as essential in the work of others and why he resists certain approaches to reading that pose problems for lumpishly categorical conceptions of schools of thought. Needless to say, what tends to get lost or at least obscured in the lumping is the significance of the differences among, and the nuances within, those grouped together, as well as the importance of the specificity or distinctiveness (not to be confused with a dubious belief in the absolute uniqueness) of an object of study or critique.

Before pursuing the issue of Jacoby’s (mis)understanding of my own work, I would note that his essay is to some extent illuminated if it is seen in the context of his recent book, *The Last Intellectuals: American Culture in the Age of Academe*.² “A New Intellectual History?” takes the theme of the book and applies it to the field or subdiscipline of intellectual history. The betrayal of the intellectuals for Jacoby has to do with their work in the academy—their scholasticism, professionalization, and specialization—and their putative abandonment of an independent, accessible role as commentators and critics in the public sphere. Bruce Robbins has

² Russell Jacoby, *The Last Intellectuals: American Culture in the Age of Academe* (New York, 1987).

subjected Jacoby's book to a strong critique.³ He points out that Jacoby relies on an ideologically suspect myth of a golden age that both conceals everything that was dubious in the older public, putatively independent intellectual and avoids much that is problematic as well as promising in the academy for the contemporary intellectual. We are after all in the academy, and the question is what we can do in the place we in all probability have chosen to be. In *The Last Intellectuals*, Jacoby provides an explicit ideal in the person of a semi-mythical independent intellectual who presumably existed in the good old days. Robbins observes:

"Independent" means selling yourself on a different market—perhaps a market that has dried up, shifted, no longer exists, as Jacoby himself more or less admits is the case for the educated reading public he then blames them [the intellectuals] for not addressing. His own book is a good case in point of the need to compromise with the market in order to sell on it. Precisely in its use of terms like "independent" it panders to the all-American ideology of self-reliance and rugged individualism . . . As a leftist, Jacoby presumably knows full well that the corporate America he thinks he's attacking has always thrived on the ideology of self-reliance and rugged individualism.⁴

In "The New Intellectual History?" Jacoby does not directly engage in golden-age mythologizing, and he even seems (projectively?) to detect it in those who have pointed to a decline or a crisis in intellectual history. He nonetheless retains the gist of his critique of the cold and bloodless academic. And the seeming implication is that some hotter, more red-blooded (independent? ruggedly individualistic? all-American?) type of intellectual (historian) is his desired hero. But the specific nature of this truly New Man (the gendered term is intentional, for the ideal type would clearly seem to be virile) and the precise kind of history he would teach or write remain a deep, dark mystery. For what carries over from the book to the article is the tendency to rely on an ideological stereotype for one's idea of an alternative, hence not explicitly to work out and argue for a normative position but instead to leave it implicit or even concealed in a claim about the way things are or were.

This last point returns me to Jacoby's (mis)understanding of what I have been trying to do. Jacoby refers repeatedly to my critique of reductive reading, and he apparently believes that this critique is the essence of my approach. I do indeed think that reductive reading represents an important problem. In fact, I think it plays a pronounced role in Jacoby's article, and this is one reason why he is sensitive to a critique. But I do not think that this critique is the essence of my work or that all forms of reduction or simplification are wrong.

THERE ARE OTHER IMPORTANT MOTIFS in my work that complement and supplement the critique of reductionism. One of them is the importance of a tense interaction between empirically based reconstruction of the past and dialogic exchange—or between the scholar and the intellectual in the intellectual historian. In fact, throughout my work, I insist on the conjunction and necessary tension

³ Bruce Robbins, "Intellectuals in Decline?" *Social Text*, 8–9 (1990): 254–59.

⁴ Robbins, "Intellectuals in Decline?" 255.

between scholarship and dialogic exchange involving critique, and I think Jacoby does not see its significance because it addresses in a different way an issue with which he is ostensibly concerned. But I think that he tends to misconstrue the issue because he conflates—or misleadingly folds into one another—empirical claims or general theories about the course of history and normative positions having socio-political dimensions or at least implications. I would not dissociate the normative from the empirical, but I would distinguish them and posit a problematic relation between them that requires elucidation and argument. In fact, if there is something essential about my work—essential in the sense not of reification or dogmatism but of sustained emphasis—it is the notion that one begins any attempt at understanding or action already situated within an interaction of forces with which one must attempt to come to terms. The great temptation is to resolve this tense interaction through analytic dissociation or speculative dialectical synthesis and to locate the solution to a problem—or one's own position—within an isolated sphere or a higher-order "reality."

Another way to make the point is to say that I am very concerned with the critique of ideology. A prominent form of ideology is the conflation of the empirical with the normative in some mode of speculative synthesis. A closely related form is the sheer analytic dissociation of the empirical from the normative and the constitution of each as a separate—and possibly essentialized or reified—realm of discourse and activity. The problem of distinguishing and cogently relating various forces or tendencies is thereby understood in a deceptively reductive and oversimplified manner. While not rejecting all modes of simplification, reduction, or readability, I do criticize misleading forms of oversimplification, ill-considered reduction, and facile readability that amount to ideological distortion. This critique goes along with the belief that acceptable simplification, reduction, and readability are among the most difficult and desirable goals of thought and practice. They are intimately bound up with the issues of translation, mediation, and supplementation. And they bear on the problem of how one should indeed relate specialized and at times necessarily difficult forms of inquiry to more accessible, altogether necessary, and desirable modes of public intercourse. Here, one must make the admittedly problematic effort to open the difficult to public scrutiny and accountability without resorting to ideologically tendentious and often implicitly elitist or disparaging modes of vulgarization. For vulgarization is not the alternative to hermeticism; it is its enemy-brother. As Robbins intimates, the task of the intellectual in the academy at the present time may well be to help create an audience for difficult ideas that one attempts to make as accessible as possible without resorting to tendentious ideological distortion. On this point, a passage from *Rethinking Intellectual History* (from which Jacoby quotes only a snippet) is apposite:

The intellectual historian should, I think, recognize his or her audience as a tensely divided one made up of both experts and a generally educated public. The intellectual historian is required to come as close as possible to an "expert" knowledge of the problems being investigated. But a goal of intellectual history should be the expansion of the "class" of the generally educated and the generation of a better interchange between them and the "experts." This means helping to put the generally educated in a position to raise more

informed and critical questions. It also means attempting to prevent expertise from becoming enclosed in its own dialect or jargon. In these senses, intellectual history faces complex problems of "translation," and its own concerns bring it into contact with larger social and cultural questions. One such question is how to resist the establishment of common culture on a relatively uncritical level and to further the creation of a more demanding common culture that, within limits, is genuinely open to contestation.⁵

I also criticize positivism as a misleading autonomization of dissociated dimensions of discourse and practice—the empirical (or "constative") and analytic dimensions. Positivism is not science but scientism that takes a restricted model of science and tries to generalize it as the only valid way to approach problems. The result is a conception of research in terms of extreme objectification of the other wherein the status of the researcher as subject is itself occluded or at least not posed as a problem. In more psychoanalytic terms, I would define positivism as the denial or disavowal of our transference relation to the object of study (including the past). In transference (as I adapt the concept from psychoanalysis), we tend to repeat aspects of the object of study in our own account of it. Thus, for example, in the study of the Holocaust, we tend to repeat processes (scapegoating, blaming the victim, disavowal, avoidance) or roles (perpetrator, victim, bystander, resister) prominent at the time, and we even face the problem of what terminology to use (Holocaust, Shoah, "final solution") in an area where no terminology is innocent or unaffected by the events themselves and by the history of their representation. We should attempt to be as aware as possible of this problem in order to elaborate more intellectually responsible and normatively controlled modes of inquiry and interaction. In research, this requires a certain combination of "objective" reconstruction and dialogic exchange in which we check our tendencies toward projection and narcissistic enclosure in order to understand the other as other and to enter into a non-invidious relation having both normative and cognitive dimensions. The critique of positivism does not, however, eliminate the need for empirical investigation to substantiate empirical claims or to test hypotheses, and I have never rejected the importance of empirical (including archival) research or the continued relevance of traditional norms of scholarship. On the contrary, the critique of positivism enables one to appreciate the virtues, necessity, and limitations of empirical investigation and to pose more cogently the problem of its relation to interpretation, understanding, and normative judgment.

I have touched on some difficult ideas as simply and concisely as I can, and I would refer the interested reader to some of my recent work.⁶ Jacoby tends to ride roughshod over these ideas. His oversimplifying and essentializing tendencies lead him to misconstrue the thought of others in a manner that precludes both

⁵ Dominick LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, N.Y., 1983), 65 n.

⁶ See especially "Is Everyone a *Mentalité* Case? Transference and the 'Culture' Concept," in *History and Criticism* (Ithaca, N.Y., 1985); and "Psychoanalysis and History," in *Soundings in Critical Theory* (Ithaca, 1989). See also my review of Arno J. Mayer, *Why Did the Heavens Not Darken? The "Final Solution" in History*, in *New German Critique*, 53 (1991): 175–92; and "Representing the Holocaust: Reflections on the Historians' Debate," in Saul Friedlander, ed., *Probing the Limits of Representation: Nazism and the "Final Solution"* (Cambridge, Mass., 1992).

careful understanding and an argumentative defense of a normative alternative or standard of critique.

Here one may also better see why Jacoby misunderstands the nature of my criticisms of important historical works such as those of Allen Janik and Stephen Toulmin, Carl Schorske, and Carlo Ginzburg. Since I cannot repeat in full my discussions of them, I would suggest that it is a useful beginning (but not an end) point of analysis to see their work as tending to share with Jacoby's the conflation of empirical claims with normative positions. In Janik and Toulmin, ethics is seen as the essential point of Wittgenstein's *Tractatus*, and this point is established not through a careful reading of the text but by an unmediated insertion of it into a fact-filled but often associatively patched-together context. English readers of Wittgenstein presumably misconstrued his basic or essential message because they were removed from his Viennese context. As I point out in my essay, a similar "misconstruction" was put forth by members of the so-called Vienna Circle, and living in Vienna was no guarantee of immunity against certain interpretations. In addition, aspects of the English context were themselves important in the writing of the *Tractatus*, including the work of Bertrand Russell and Lewis Carroll. I do not argue that the *Tractatus* is ultimately about nothing, although I do think that it runs the risk of silence as do many if not all truly ambitious ventures in modern thought. But this risk should not be fixated upon or converted into the pretext for an excessive and fetishized ideology of the sublime. Nor should silence be mystified or seen as the hidden telos of language; it is intimately bound up with the use of language, and how language breaks up or breaks down is crucial to the appreciation of the nature of the silence that ensues.

In addition, I emphatically do not dismiss interpretation. I try not only to understand how it works but to engage in it and relate it to more intricate issues in reading. My basic argument here as elsewhere is that contexts are indeed important for interpretation and reading but that they are multiple and at times conflicting or at least problematically related to one another as well as to interpretation and reading. Any assumption that one context or set of contexts is particularly significant (or "essential") must be made explicit and argumentatively defended, for it is contestable. Moreover, one crucial question is precisely how texts come to terms with the various contexts bearing on them. Even stereotypical, formulaic, vulgarizing texts (such as political propaganda, commercial advertising, and pulp literature) do not simply reflect or illustrate a context but reproduce it with typically legitimating ideological effects. Indeed, no text totally masters its contexts or transcends a more or less unconscious implication in contemporary ideologies, although some texts are obviously more critical than others in the way they engage contexts and ideologies. The general question one may pose to any text is how precisely it relates in symptomatic (or ideologically reinforcing), critical, and potentially transformative ways to its various pertinent contexts of production and reception. To raise this question is not to glorify a textual technique but to stress the importance of cognitive responsibility and the willingness to defend interpretations in explicit and argumentatively developed ways. Texts are both historical events in their own right and a crucial basis for our

inferential reconstruction of other events; the problem of how to read and interpret them should be considered vital for the historian.

In the case of the *Tractatus*, it is quite implausible to see ethics as the essential message of the text—except perhaps from a normative position. But then, at the very least, the normative position must not be left implicit or smuggled into the interpretation; it must be elaborated, elucidated, and defended in a manner that is not attempted in the book by Janik and Toulmin. Indeed, the understanding of ethics in the book is individualistic and bound up with an uncritical mystique of “silence.” It is by and large removed from politics and society as well as from the issue of how the individual and the socio-political interact in ethically relevant fashion.

In Schorske's case, the crisis of a liberal polity is presumably the essential core of the *fin-de-siècle* Viennese crisis, while in Ginzburg's, the essence of Menocchio's world-view is a reading code rooted in millennial peasant culture. I tried to argue that Schorske's argument was in part projective with respect to the United States of the 1950s and that it was misleading to center the Viennese crisis on liberalism. Schorske's approach obscured certain dynamics in Viennese culture, and it enabled him to privilege liberalism without engaging in a sustained, discriminating analysis or defense of its nature and its relations over time to other political and social options. It also induced oversimplified interpretations, such as that of Freud as basically a frustrated liberal who escaped from politics. In Ginzburg's case, a popular peasant reading code that is ideologically central for Ginzburg is perceived as central to Menocchio. I pointed out that Menocchio was both peasant and miller in addition to being a marginal figure in certain respects. Moreover, artifacts of high culture were manifestly important for him, and he wanted to impress the “higher-ups.” The more crucial point is that what appeared from Ginzburg's own evidence to be very significant in Menocchio's case was the interaction or even hybridization of levels or dimensions of culture both within the larger society and within the personality of Menocchio himself. Here (as elsewhere), I was trying to raise the issue of both the actual and the desirable role of hybridization in culture. I also tried to pose the problem of hegemony, which involves the internalization of dominant norms and values by subordinate groups, a problem with respect to which the idea of an autonomous peasant culture or reading code may well be wishful thinking. I found certain aspects of Ginzburg's peasant-populism to be both empirically dubious and normatively questionable. In any case, I thought that one had to distinguish between the empirical and the normative in order to understand their relations in the past and to argue for their desirable reconfiguration in the present and future. To avoid such issues is to be prone to the most unreflective and unself-critical mode of ideology. Indeed, the very point of my claim that “the reader deserves a transcription of [or, more realistically, significant quotation from] the inquisition register” on which Ginzburg bases his interpretation is to insist that social history be more publicly accountable by providing the reader with a basis for critically evaluating and possibly contesting interpretations and readings.⁷ Public accountability becomes

⁷ Dominick LaCapra, “*The Cheese and the Worms: The Cosmos of a Twentieth-Century Historian.*” in LaCapra, *History and Criticism*, 63.

impossible when the archive by and large remains the secret repository on which a narrative or an analysis is elaborated. It is astonishing that Jacoby, despite his plea for the public and accountable intellectual, would take exception to this point or simplistically reduce it to little more than a bromide about textual prudence and greater caution.

I SHALL SAY ONLY A BRIEF WORD about Jacoby's massive misconstruction of my essay "On Grubbing in My Personal Archives: An Historical Exposé of Sorts (or How I Learned to Stop Worrying and Love Transference)." The title alone would indicate that this venture is intended as parodic and (quite importantly) self-parodic. One may judge it to be in poor taste, and it certainly does raise the issue of taste by explicitly and intentionally testing the nature of professional norms of decorum and their relation to "personal" and biographical issues. But it is difficult to see how one could simply read it straight, as Jacoby does. It may seem "odd" to one who lacks a sense of humor and irony. In any case, it is hardly the simple expression (or "posturing") of a well-situated academic who takes "umbrage" at the audacity of an upstart assistant professor who refuses to "back down." In fact, I thought that the review was not audacious insofar as it chimed with what I claimed were conventional or even hegemonic voices in the historical profession. One may want to take issue with my claims or the style in which they are conveyed, but one must see them in the context of an essay that interrelates argument with more indirect modes of address such as irony, parody, and hyperbole. Furthermore, one may perhaps suspect a little posturing on Jacoby's part in his manifest inclination to identify with the putative underdog and to frame the issue in straightforward if not storybook terms.

One of the most important issues that emerges from a reading of Jacoby's "New Intellectual History?" is the relation between acceptable reduction or simplification and misleading oversimplification or ideological distortion. Distortion is always bound up with personal investments. It is also linked to the question of the role of the intellectual and the conditions created by work in the academy. One implication of Jacoby's analysis is that certain kinds of complexity may themselves be ideologically distortive, misleading, or diversionary, and I would agree with this point. But I would also insist that one examine carefully whether the level of difficulty in an account is warranted by the difficulty of the subject matter and whether there is an attempt made to effect—or at least to pose the problem of—cogent translations between different modes of discourse and areas of culture. As I noted at the outset of this response, Jacoby is known as someone who has tried to revive the spirit of critical theory and apply it to contemporary conditions in the academy and beyond. I thoroughly support and in fact actively try to further this project in my own work. But the question I would raise is whether aspects of Jacoby's critique of the "new intellectual history" accord too readily with recent neoconservative reactions that deride those difficult theory-people and clamor for a return to a supposed golden age when the canon was the canon and life was simpler and more straightforward. In its bearing on the issue

of the broader political resonance of academic debate, this question should at least make one pause and reflect.

Beyond more circumscribed political issues, Jacoby's approach converges with the unfortunate tendency in some recent commentators to become familiar with theoretical perspectives only to be better able to criticize and fend them off. This rather unconstructive and defensive strategy leads at best to containment by partial incorporation and to rather unreflective tensions in the reactions of the negative critic. In the recent past, this tendency has characterized the work not only of professional historians (for example, Donald R. Kelley, James T. Kloppenberg, and Anthony Pagden in the essays to which Jacoby refers) but also of literary critics who, at times under the banner of a "new historicism" or a "new pragmatism," have utilized theoretical sophistication to advocate a movement "against theory."⁸ This bizarre form of anti-theoretical theory may lead to the intentionally unearth-shaking conclusion that theory makes no difference in practice, that it amounts to spinning one's wheels in the void—a conclusion that may indeed apply to some forms of contemporary theory, prominently including anti-theoretical theory.

Jacoby's essay is, unfortunately, of little value in offering insight into the problem of fruitfully conjoining history with theory. I would like to turn at this point to a more constructive engagement with problems I think Jacoby does little to elucidate.

HOW SHOULD ONE UNDERSTAND the conjunction of history and theory? First, the relation between history and theory should not be seen as a mere additive relation or associative link. The idea of "history and theory"—a title that in fact graces an important journal—may authorize a mere assemblage of reflections on history from a rather conventional perspective and on theory from relatively ahistorical or narrowly analytic points of view. The relation between history and theory should be dialogic and mutually provocative—a relation in which the terms are inter-involved and in part transformed by their mutual implication.

Second, the conjunction of history and theory implies a critique of history without theory or history in which the theoretical component remains implicit. The latter approach has often characterized conventional historiography, and it has engendered the idea of history as a craft. The historian's craft, in Marc Bloch's phrase, has indeed produced much admirable work, and I have already intimated that I would in no sense want to jettison the norms of meticulous research and careful testing of propositions that have become ingrained as common sense in the historical profession. In fact, I think that even the most theoretically sophisticated and experimental approach should have more than a nodding acquaintance with common sense and with disciplinary traditions that can test and

⁸ See, for example, Steven Knapp and Walter Benn Michaels, "Against Theory," in W. J. T. Mitchell, ed., *Against Theory: Literary Studies and the New Pragmatism* (Chicago, 1985), 11–30. Interestingly, Kloppenberg also turns to pragmatism to support his critique of recent theoretical initiatives, and there is at least a partial convergence between some recent literary criticism and historiography in the tendency to combine contextualizing historicism with a pragmatic hermeneutic.

contest its more speculative ventures. But the procedures of established disciplines such as professional historiography should also be rendered more explicit and thus more open to questioning, revision, and supplementation. Otherwise, the misleading understanding of history as an alternative to theory may well induce one to hypostatize history, essentialize context, and confide in an unmediated idea of the manner in which "historical" information purports to explain various features of texts.

Third, conjoining history with theory does not lead to theory without history or, more precisely, to theory in which the historical dimension is extremely attenuated, abstract, and unspecified. For, just as there is in reality no history without theory, there is also no theory without history. But the relations between the two may be implicit or even repressed with the result that the problems and the potentials of a more explicit and critical relationship may be obviated or misconstrued. Although there is always the risk that theory will develop beyond—or fall short of—its object and become self-referential, the relationship between theory and history should not be seen solely in terms of a self-propelled theoretical movement that engenders its own internal resistances or that construes history in extremely theoreticist and easily misleading terms such as referential aberration, aporetic (skeptical) impasse, and radical discontinuity or fragmentation. The latter view (often associated with the name of Paul de Man, with whom Jacoby at one point seems to confuse me)⁹ accords with the recent fixation on an (an)aesthetic of the sublime,¹⁰ and I have already intimated that in my judgment it tends to fetishize or compulsively repeat what is indeed one important and unavoidable possibility in thought. The larger problem is, however, to explore the interaction between various dimensions of language use and its relation to practice, including the relationship between "constative" historical reconstruction and "performative" dialogic exchange with the past as well as between "sublime" excess and normative limits that are necessary as controls in social and political life.

In line with the effort to conceive of history with theory, one should make a sustained attempt to relate the reading of texts and artifacts to specific historical and socio-political questions. In fact, the problem of specificity is vitally at issue in the three implications I have drawn from the conjunction of history and theory. One should also try to indicate precisely how historiographical studies and debates might profit from closer, more critical attention to rhetorical and textual matters and to the kinds of theory that provide perspective on these matters. Here as elsewhere, one needs what I have referred to as a translation between disciplines and areas of culture. But any effective translation must be sensitive to the different traditions and protocols of interpretation within disciplines or areas of culture. A translation that is premised on the understanding of only one

⁹ For a critique of certain tendencies in Paul de Man's work, see my "Temporality of Rhetoric," in LaCapra, *Soundings in Critical Theory*. Jacoby's confusion of my approach with at least a vulgarized idea of de Man's occurs when he believes I argue that the *Tractatus* "is essentially about nothing (or is essentially nothing)." Jacoby, "A New Intellectual History?" 417.

¹⁰ See, for example, Jean-François Lyotard, *Heidegger and "The Jews,"* Andreas Michel and Mark S. Roberts, trans. (1988; Minneapolis, Minn., 1990).

tradition (such as professional historiography, on the one hand, or deconstruction, on the other) is necessarily an insufficiently complex, at times one-dimensional, appropriation that fails even to register as relevant in the terms of those within the other interpretive tradition. One should not, however, value complexity for its own sake or essentialize “high” culture as the sole sanctuary of resistance in an administered society.

Defensible complexity is related to dialogism in the basic sense of the interaction of mutually implicated yet often contestatory traditions or tendencies that have provocative relations to one another. These traditions or tendencies indicate why texts in which they are at issue cannot be reduced to mere symptomatic documents insofar as texts perform critical and transformative work or play on their contexts of production and reception. In this sense, these texts—texts such as those of Marx, Freud, or Wittgenstein—demand a response from the reader that should not dispense with, but cannot be confined to, contextualization. In addition, contexts themselves may well involve mutually contestatory tendencies that significantly complicate the problem of relating them to texts. But there is no need to postulate a dichotomy or simple choice between an interest in texts and in contexts, although the work of different historians will legitimately show different stresses and strains in addressing them. Nor need one accept a semiological theory in which language is conceived of as a self-contained system of signs—a theory that both Voloshinov and Derrida criticize as a formalistic idealization.¹¹ Highly dialogized texts and contexts may, however, be argued to require a dialogic and self-critical response from the reader that is intimately related to the subject-positions he or she occupies and is attempting to forge. The basic point here is that one should not hypostatize the text, the context, or the reader but attempt to understand the relations among them in tensely interactive terms. Even more basically, one should construe one’s own position as inserted within that interaction in relation to which text, context, reader, and subject are themselves more or less useful abstractions.

Rethinking Intellectual History (1983) engages various old historicisms whose importance in professional historiography is not entirely a thing of the past. The book attempts to give a significantly different twist to a traditional approach to a problem that was and is familiar within intellectual history, for it often relies on established canons but turns to critically noncanonical readings of canonical or, more precisely, canonized texts. In later work, I render more explicit and to some extent revise the strategy employed in *Rethinking Intellectual History*. It is, I think, important to distinguish between canonization—a basically conservative practice in the reception or appropriation of artifacts—and the potentials of those artifacts to be brought out through critical readings that, in Walter Benjamin’s words, brush history against the grain. It is only through an essentializing and misplaced ritual process that one apprehends canonization as totally and irredeemably

¹¹ See V. N. Voloshinov, *Marxism and the Philosophy of Language*, Ladislav Matejka and I. R. Titunik, trans. (1930; New York, 1973), especially the critique of Ferdinand de Saussure on 58–61; and Jacques Derrida, *Of Grammatology*, Gayatri Chakravorty Spivak, trans. (1967; Baltimore, Md., 1976), especially the discussion of Saussure on 44 and following. There was a very close relationship between Mikhail Bakhtin’s views and *Marxism and the Philosophy of Language*, although scholars debate whether or to what extent Bakhtin actually wrote the book under the name of Voloshinov.

contaminating texts or artifacts. By contrast, it is necessary to understand canonization critically as a historical process through which texts are made (however problematically) to serve hegemonic interests in ways they both invite and resist more or less compellingly. The process of canonization requires that the critical or even potentially transformative—noncanonical or anticanonical—dimensions of texts and other artifacts be repressed or radically downplayed.

JACOBY NOTES THAT, FOR MANY OBSERVERS, intellectual history seemed to be in the doldrums in the 1970s and early 1980s. Interest, momentum, and talent had apparently shifted to social history, and intellectual history as a field was often felt to be moribund. Yet I think it was, in at least limited fashion, an important locus for theoretical self-reflection in historiography and a noteworthy point of entry for more critical forms of reading and interpretation (prominently including noncanonical readings of canonical texts) often coming from Continental Europe. The enterprise of reinvigorating it seemed worthwhile to a number of professional historians. By now, the enterprise has largely succeeded: intellectual history is alive and kicking, and its theoretical concerns are being discussed in other areas of historiography and entering into mutually thought-provoking relations with other disciplines.

For intellectual history to achieve its present reinvigorated status, it was necessary to reconsider certain prevalent assumptions about its nature and to counter ill-considered attacks on it. But its renewed and more secure standing may now actually aid informed attempts to rethink the subdiscipline in even more basic terms and to relate it with greater insistency to a more encompassing mode of inquiry into culture and society. Critical, noncanonical readings of canonized artifacts remain, I think, pertinent for somewhat different reasons both in history and in other disciplines. But one's larger ambition should be the elaboration of more intellectually and politically significant modes of cross-disciplinary exchange having a bearing on institutional change in the academy and beyond it. One should also supplement and problematize noncanonical readings of canonized texts with a broader rethinking of the canon and the problem of canonicity in general.

Rethinking the disciplines in basic ways is a more difficult and risky venture than certain of its proponents at times seem to believe, and it is all too easy to call for basic intellectual and institutional revolutions that in reality amount to a relatively simplistic reformulation of older views whose genealogy and even whose existence one may ignore. Thus it is no great transformation or "instauration" to call for an evangelical "new" historicism in which salvation is sought through an empathetic understanding of the "other" purely and simply in his or her own time, terms, and place. It is nonetheless the case that any significant change in older modes of canonical interpretation involves an insistent attempt to raise the issues of race, class, gender, sexual orientation, and species as well as to explore historically and critically the emergence and function of "levels" or areas of culture in society. It also entails the possibility that intellectual history—along with

other “humanistic” disciplines such as literary criticism—ought to be more consistently related to a differently articulated program of cultural studies in which the study of popular culture is not detached from that of “elite” or “high” culture. Any acceptable program would not imply that one simply cashiers formerly canonical texts but that one reads them differently and relates them to the larger problem of critically inquiring into the formation and function of a canon in the production and reproduction of a stratified and conflict-ridden society and culture. For, unless one construes cultural studies solely in terms of a necessary but, in my view, far from sufficient program for a historical sociology of culture, the issue of what and how one reads remains crucial.¹² Here I would reformulate somewhat the argument I made in *Rethinking Intellectual History* in terms of a distinction between texts that are good to think with and texts that are good to think about.

All texts are good to think about, and no text is ideal. Indeed, all texts are hybrids or compromise-formations involving an interaction between ideologically reinforcing and more critical tendencies. But some texts are especially good to think with precisely because they counteract blindly symptomatic processes of ideological reinforcement in and through critical and self-critical movements. Texts that are especially good to think with should not be seen as “privileged” in an invidious sense; they should be viewed as educationally and politically valuable in part because they test theories while providing some perspective on their own workings and on larger socio-cultural processes. They may indicate that the difficult problem in both theory and social practice is how to articulate various differences in non-invidious but (to varying degrees) normatively regulated networks of relations. Still, the debatability of the very norms by which one evaluates desirable textual formations indicates that one must continually rethink the issue of which texts are especially good to think with and to employ in acquiring ways and means of coming to terms with more decidedly symptomatic texts and cultural processes. Moreover, the larger intellectual and practical goal is to elaborate an approach to society and culture in which the reading of texts is one crucial element but not the only or even the most important concern.

From what I have argued, it should be evident that it is indeed vital to attend to the way more symptomatic artifacts are able to reinforce ideological needs and desires or even to hold out a more or less distorted image of utopia. But it is equally vital to elaborate an approach in which it is possible to address this problem critically, and certain texts may be particularly valuable in cultivating this approach. There is no simple formula that will enable one to decide which texts these are, but the process of education—and of educating oneself as an educator—requires that this be a topic that is recurrently debated. One might call it the problem of reformulating expanded and revised critical “canons” that are open to questioning and self-questioning, particularly with respect to formerly excluded

¹² This issue is at times obscured in recent analyses of canonicity, including the important contribution of John Guillory, “Canonical and Non-Canonical: A Critique of the Current Debate,” *ELH*, 54 (1987): 483–527. On canonicity and related issues, see the essays in Dominick LaCapra, ed., *The Bounds of Race: Perspectives on Hegemony and Resistance* (Ithaca, N.Y., 1991). See also my “Canons and Their Discontents,” *Intellectual History Newsletter*, 13 (1991): 3–14; also included in Lloyd Kramer, Donald Reid, and William Barney, eds., *Learning History in America* (Chapel Hill, N.C., forthcoming).

artifacts of women, minorities, and culturally subordinated groups in general. Dialogic relations both within and between disciplines, texts, groups, and selves have a continually renewed role to play in eliciting and elaborating different positions of vantage on textual and cultural processes.

It might still be plausible to suggest that, at least in the recent past, certain texts of "high" culture have had a particularly powerful critical charge in part because they have not entered fully into the commodity system and are instead objects of relatively small capital investment. (To account for that critical charge, Adorno's at times misleading ideas about "autonomous" art and the role of negative textual space in an administered society would have to be revised in important or even drastic ways, notably in the direction of a better understanding of the divided tendencies within any text or artifact and the interactions among various artifacts in different "levels" or areas of culture.) Mass culture, by contrast, is heavily capitalized and commodified, and popular culture in industrial societies has been largely absorbed into the commodity system. The fact that we often use "popular" and "mass" culture interchangeably is one index of the extent to which popular culture has been integrated into the commodity system. This indiscriminate usage is distortive and anachronistic with respect to other forms of popular culture that existed and functioned under significantly different conditions and might at times have critical, transformative, or legitimately affirmative tendencies. But, even with respect to more recently commodified popular and mass culture, we should avoid blanket categorizations or condemnations that always skirt essentialization, elitism, and self-defeating cultural pessimism. Instead, we should attempt to work out sustained and careful analyses of the way artifacts always affect social and cultural stereotypes and ideological processes, even when they forcefully reproduce and reinforce banality.¹³ We should also be actively alert to how artifacts of mass culture may indeed have popular and even critical dimensions either through creative modes of consumption or through more thoroughgoing and even collective procedures in which commodified artifacts are reproduced or "refunctioned."¹⁴

In addition, it is difficult to deny that the counterpart to the relative resistance of aspects of culture to commodification is frequently their hermeticism, including their tendency to recycle older and more popular forms, such as the carnivalesque, in largely inaccessible ways. At times, this difficult or hermetic quality may be justifiable.¹⁵ But it is also possible—particularly when certain

¹³ For a discussion of some of these issues, see Alice Yaeger Kaplan, *Reproductions of Banality: Fascism, Literature, and French Intellectual Life* (Minneapolis, Minn., 1986); and "Paul de Man, *Le Soir*, and the Francophone Collaboration (1940–1942)," in Werner Hamacher, Neil Hertz, and Thomas Keenan, eds., *Responses: On Paul de Man's Wartime Journalism* (Lincoln, Neb., 1989), 266–84. See also my essay, "The Personal, the Political, and the Textual: Paul de Man as Object of Transference," *History & Memory* (forthcoming).

¹⁴ See, for example, Andrew Ross, *No Respect: Intellectuals and Popular Culture* (London, 1989); and Lawrence Grossberg, Cary Nelson, and Paula Treichler, eds., *Cultural Studies* (London, 1992). I would like to note what should be obvious. A critical process need not be purely negative. It may suggest alternatives. It may also revise or even reinforce established principles and policies to the extent that they are able to stand up to criticism and prove worthy of affirmation. But then their reproduction is not simply ideological.

¹⁵ One of the more interesting and valid reasons for the appeal of a difficult or hermetic style as a strategy of resistance is given by the Tunisian writer Abdelwahab Meddeb: "We will defend

strategies have become routinized—that texts employing them, even when they attempt to subvert the high/low opposition and explicitly attack both the stratification of levels of culture and their own “high” or “elite” status, may be elitist, for example, in their function as symbolic capital and social reinforcement for a restricted in-group or *cénacle*. Whether such a state of affairs is compatible with the type of democratic values and politics often advocated by relatively hermetic critics poses a significant problem.

This problem is not confined to any one theoretical tendency, and its sources are bound up with advanced forms of the division of labor both in the academy and in the larger society. But an awareness of the problem should at least indicate that the necessary difficulty required by rigorous and self-critical thought should not itself be fetishized or correlated with some essentialized ranking of types of thought such as that which presents accessibility as necessarily a feature of a degraded or pejoratively vulgar thinking. One aspect of traditional historiography that is indeed worth preserving in a transformed manner is the idea that all forms of writing—and certainly all forms of academic writing in the humanities—should ideally make contact with diverse social groups. This ideal requires a style of address and a type of social reconstruction that should be affirmed, however difficult they may be to elaborate in a sustained and cogent fashion. Another directive one may take up and reformulate from traditional historiography is that we should insistently relate the reading of texts and other artifacts to the interpretation of significant broader problems in history and social life. The obvious questions are how this reformulation should be undertaken and the import it has for the elaboration of theory that is not self-contained but open to a sustained interchange with historical research.

ourselves with arabesque, subversion, labyrinthine constructions, the incessant decentering of the sentence and of language so that the other will lose the way just as in the narrow streets of the *casbah*”; quoted in Jean Déjeux, *Situation de la littérature maghrébine de langue française* (Alger, 1982), 103–04. Compare Theodor Adorno: “What everybody takes to be intelligible is in fact not intelligible at all. Conversely, what our manipulated contemporaries dismiss as unintelligible secretly makes very good sense to them indeed. This recalls Freud’s dictum that the uncanny is uncanny only because it is secretly all too familiar, which is why it is repressed . . . The accessibility of past art spells its doom. To validate this, one only has to look at the fact that there are many dark and doubtlessly misunderstood works among those enshrined for ever in the pantheon of classics.” Adorno, *Aesthetic Theory*, C. Lenhardt, trans. (1970; London, 1984), 262.

Review Article
The Use and Abuse of *Black Athena*

MOLLY MYEROWITZ LEVINE

Martin Bernal, **Black Athena: The Afroasiatic Roots of Classical Civilization**, Volume 1: **The Fabrication of Ancient Greece, 1785–1985** (New Brunswick, N.J.: Rutgers University Press, 1987). 575 pp. \$50.00, paper \$15.00.

Akavyah ben Mahalalel said: Reflect upon three things, and you will not come into the grip of sin: Know whence you came, where you are going, and before Whom you will have to render account and reckoning. Whence you came?—from a putrid drop.

Pirkei Avot 3.1.

VOLUME 1 OF *Black Athena* IS AN EXTRAORDINARILY INTERESTING and dangerous book: interesting in its sweeping sociology of the historiography of early Greece from the fifth century B.C.E. to the present; dangerous, because in reopening the nineteenth-century discourse on race and origins, the work, sadly, inevitably, has become part of the problem of racism rather than the solution that its author envisioned.¹

About the time Virgil was writing his *Aeneid*, a Palestinian rabbi named Akavyah ben Mahalalel moralized against the glorification of the human species by focusing on the humble begetting of individual human beings. His blunt answer (preserved in Mishnah *Avot*) to the question of origins—“from a putrid drop”—comes as a healthy and universalist corrective to a multitude of particularist mythologies of “in the beginning.”² Never content to leave it at that, humankind,

I owe my title and inspiration to M. I. Finley, *The Use and Abuse of History* (1975; rpt. edn., New York, 1987), particularly to Finley's essay “Generalizations in Ancient History,” 60–74. Thanks to Martha Davis, Art Eckstein, Tamara Green, Rudolf Hock, Marilyn Arthur Katz, Moti Manovitz, Erich Martel, Charles Mercier, Rosaria Munson, Victoria Pedrick, Jack Peradotto, Michael Poliakoff, Amy Richlin, Alice Riginos, Alex Tulin, and the Howard and Vassar Classics Departments for their generous help. This essay is dedicated with profound admiration to Professor Frank M. Snowden, Jr., who, from the wealth of both his scholarship and life experience, taught me to understand that “not everybody is a racist.” Needless to say, all opinions are my own.

¹ Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, Volume 1: *The Fabrication of Ancient Greece, 1785–1985* (New Brunswick, N.J., 1987). On the author's political purpose, see discussion below.

² The Mishnah was redacted toward the end of the second century C.E. but contains much earlier material. Akavyah ben Mahalalel, who was excommunicated for his iconoclasm but admired for his

scholars included, has spun stories, histories, and mythic histories of origins. Motives have ranged from the purely speculative to the overtly political, but always there is the sense that within the answer to the riddle of the Sphinx—the secret of birth—is buried the truth of identity: who we were born (we imagine) will tell us who we are. Surely, of all possible approaches to the question of origins, race has proven historically to be the most fruitless, divisive, and destructive, in ways that the framers of the Mishnah were unable to anticipate.

It is precisely this question of origins that is the focus of Bernal's *Black Athena* series, in which the author, a scholar of Chinese history and government, professor of Near Eastern studies, and self-avowed "outsider," lays serious and sweeping charges against what he sees as the "classics establishment," the "insiders." These, claims Bernal, have suppressed or ignored the weight of what he considers to be overwhelming evidence for Egyptian and Semitic contributions to the origins of ancient Greek civilization. The true origins of the Greeks, argues Bernal, lie in a fruitful mixture of many Mediterranean and European peoples. Although, in his search for ancient origins, Bernal makes a valiant attempt to negotiate the maze of modern concepts and ideologies of race and culture, ethnic and language groups,³ he may have succeeded in only making matters worse: too many readers seem not to have followed his lead.

The Fabrication of Ancient Greece, the first volume of the *Black Athena* series,⁴ traces the shift in Greek historiography from the Greeks' own ancient model of their prehistory as a synthesis of Pelasgian (Indo-European), Phoenician (West Semitic), and Egyptian elements, what Bernal calls the "Ancient Model," to a nineteenth-century paradigm that at its most extreme explained Greek civilization as the result of a conquest of native "Pre-Hellenic" peoples by Indo-European speakers, Bernal's "Aryan Model." These Pre-Hellenes, while not Indo-European speakers, were white, definitely not Semitic or African. According to Bernal, classical scholarship used this "Aryan Model," born at least in part of the new spirits of Romanticism, racism, and progress, to deny any fundamental role to Canaanites or Egyptians in the making of Greek civilization. In its heyday, from 1927 to the 1960s, the most extreme version of the Aryan Model was used to expunge systematically all the non-Indo-European influences on Greece seen by earlier scholars. Since then, the less extreme "Broad Aryan Model," which cautiously admits some influence from the Levant while denying any outright influence from Egypt, has made a comeback as a result of two factors: new archaeological discoveries and the ebb of anti-Semitism in academia.

But we have not come back far enough, argues Bernal, who wants to reinstate what he calls the "Revised Ancient Model." This accepts the nineteenth-century

intellectual integrity (*Mishnah Eduyot* 5.6–7), is traditionally dated to the time of Hillel the Elder at the end of the first century B.C.E.; R. Travers Herford, *The Ethics of the Talmud: Sayings of the Fathers* (New York, 1962), 64.

³ See especially Bernal, *Black Athena*, 1: chap. 4, "Hostilities to Egypt in the 18th Century," and chap. 5, "Romantic Linguistics."

⁴ Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, Volume 2: *The Archaeological and Documentary Evidence* (New Brunswick, N.J., 1991), the second volume of a projected tetralogy. For vol. 2, focusing on archaeological and documentary evidence, see the review by Robert L. Pounder below.

identification of Greek as an Indo-European language and its implications of northern influence on Greece at some early stage, but it also accepts the ancient stories of Egyptian and Phoenician settlements in the Aegean and maintains that these as well as other contacts in the Bronze and Iron Ages led to massive Afroasiatic cultural influences on Greece. Thus most of what supporters of the Aryan Model call the non-Indo-European or Pre-Hellenic aspects of Greek civilization can more usefully be seen as Egyptian or West Semitic. Bernal believes that classicists have not noted what he believes to be obvious because of residual racism and anti-Semitism and, far more important, the academic inertia and respect for authority that is widespread in all disciplines.

Armed with a formidable aegis of scholarly apparatus, Volume 1 of *Black Athena* seeks to convince us of five major points. First, taken together, the ancient evidence from the fifth century B.C.E. to the fifth century C.E. indicates that classical, Hellenistic, and later pagan Greeks recognized their debt to Egypt and Phoenicia, in other words, that there was indeed a relatively monolithic Ancient Model. Second, this debt to Egypt and the Levant remained an unbroken part of the European historiographical tradition on Greek origins until the beginning of the eighteenth century. Third, during the eighteenth and nineteenth centuries and coincident with the establishment of "classics" as a modern academic profession, first the Egyptian and then the Phoenician contributions to Greek civilization were denied. Partly due to the intrinsic evidence of Indo-European linguistics but mainly to extrinsic European social and intellectual forces, the Ancient Model was supplanted by the Aryan Model. Fourth, although Semitic influences have been increasingly acknowledged, the ancient tradition of Egyptian influence on Greece is still denied, again for extrinsic reasons; thus a Modified Aryan Model persists in classical scholarship and teaching today. Finally, although "conception in sin" does not invalidate the Aryan Model as a heuristic scheme, it does call its inherent superiority into question.⁵

Although the focus of *The Fabrication of Ancient Greece* is historiography—ancient Greek versus modern European historiography, and the sociology of classical knowledge that created, permitted, and preserved the Aryan Model as a historical paradigm—the first volume of *Black Athena* also marshals considerable epigraphic, linguistic, and archaeological evidence to support Bernal's own mandate for historical revision. Future volumes (the now published Volume 2 presents mainly archaeological and documentary evidence, with language, toponyms, religion, and mythology largely reserved for Volumes 3 and 4) promise to test the plausibility of competing historical models against documentary, archaeological, linguistic, iconographic, and mythological evidence in greater detail. Bernal bases his arguments for historical revision on what he calls a principle of "competitive plausibility," which James Muhly (one of Bernal's chief bad guys and most outspoken critics) recently has dubbed "competitive but not very plausible."⁶

⁵ Bernal, *Black Athena*, 1: 439–43, quote 442.

⁶ Bernal, *Black Athena*, 1: 7–9, 2: 3–4; James D. Muhly, "Where the Greeks Got Their Gifts," *Washington Post Book World* (July 21, 1991): 4. For Bernal on Muhly, see *Black Athena*, 1: 421–22, 426–27, 432; 2: 399–402, 468–69 *et passim*. For Muhly on Bernal, see "Preface" and "Black Athena versus Traditional Scholarship," *Journal of Mediterranean Archaeology*, 3 (1990): 53–55, 83–110; and Muhly's lecture "Is There Evidence for Egyptian Colonization in Central Greece?" at the Temple

The proposition that there may be two equally plausible explanations for the same data, however troubling, may on occasion be the best we can do in the study of antiquity, especially in prehistory, where fragmentary evidence often yields no decisive final answer. Frustration alone is enough to account for the fact that refutations of conclusions based on plausibility can take the form of simple dismissal ("I am not persuaded"), as is frequently the case with Bernal's arguments, the responses of his critics, and Bernal's responses to responses (particularly in the area of etymologies).⁷ Furthermore, competitive plausibility worked well as a *modus operandi* for Volume 1, where it functions as something of a conceit for the author's often deliberately disingenuous contrast of "Ancient" and "Aryan" models. But to argue that two models are competitively plausible, or offer equally good explanations for the extant "intrinsic" evidence, fails as a proof for Bernal's own historical model. Bernal wants his Revised Ancient Model (with its attendant radical revisions in Aegean and Egyptian chronologies) to be judged by the traditional rules of the game in which a hypothesis stands or falls on the basis of whether it offers a better or worse explanation of the extant evidence. Accordingly, despite the many confusing allusions to plausibility throughout Volume 2, Bernal himself seems now to have moved at least in principle from his earlier posture of competitive plausibility, with its implications of neutrality, to a more conventional scholarly mode of argumentation: "I have given up the mask of impartiality between the two models . . . Now, instead of judging their competitive heuristic utility in a 'neutral' way, I shall try to show how much more completely and convincingly the Revised Ancient Model can describe and explain

University Symposium on *Black Athena*, "Challenging Tradition: Cultural Interaction in Antiquity and Bernal's *Black Athena*," October 19–20, 1991, co-sponsored by the departments of Classics and African American Studies. Muhly's lecture and the presentations of other participants referred to below are available (edited on audio and videotape) from Martha A. Davis, Project Coordinator, Dept. of Classics, Anderson Bldg. (022–34), Temple University, Philadelphia, PA 19122.

⁷ For critiques together with Bernal's responses, see *The Challenge of "Black Athena"*, special issue, *Arethusa* (Fall 1989), M. M. Levine and John Peradotto, eds.; M. M. Levine, "Classical Scholarship—Anti-Black and Anti-Semitic?" *Bible Review*, 6 (June 1990): 32–36, 40–41; "Discussion and Debate" sections of *Journal of Mediterranean Archaeology*, 3, nos. 1–2 (1990); *Black Athena* (a television film, available in the U.S. from California Newsreel, 149 9th Street, 420, San Francisco, CA 94103); Bernal's response to Edith Hall, "When Is a Myth Not a Myth? Bernal's 'Ancient Model,'" both in *Arethusa*, 25 (1992): 181–201, 203–14.

"I am not persuaded" has, to date, been the general response to Bernal's arguments for Semitic (25 percent) and Egyptian (20–25 percent) roots for Greek words; see Sarah Morris, "Daidalos and Kadmos: Classicism and 'Orientalism,'" in *Challenge of "Black Athena"*, 39; compare Jasper Griffin, "Who Are These Coming to the Sacrifice," *New York Review of Books* (June 15, 1989): 27; John Ray, "An Egyptian Perspective," *Journal of Mediterranean Archaeology*, 3 (1990): 80–81; and Michael Poliakoff, "Roll over Aristotle: Martin Bernal and His Critics," *Academic Questions* (Summer 1991): 14. Griffin, Ray, and Poliakoff find Bernal's etymologies difficult to judge since "he exploits resemblances that seem, so far at least, not to be a matter of laws but ad hoc in each case"; Griffin, 27. For Frank Yurco, despite Bernal's seductive cognates between Egyptian and Greek, "cognates alone . . . do not make a case for close linguistic relationship"; Yurco, "Letter to the Editor," *Chronicle of Higher Education* (September 4, 1991), B4. Thus the burden of linguistic proof still rests on Bernal until his full linguistic arguments appear in Vol. 3. The only extensive and generally favorable treatment in print of Bernal's etymologies thus far comes from Gary Rendsburg, "Black Athena: An Etymological Response," in *Challenge of "Black Athena"*, 67–82; with Bernal's response, Martin Bernal, "Black Athena and the APA," in *Challenge of "Black Athena"*, 32–37. On the peculiar situation of linguistic criticisms of Bernal, see M. M. Levine, "The Challenge of *Black Athena* to Classics Today," in *Challenge of "Black Athena"*, 12–14.

the development and nature of Ancient [*sic*] Greek civilization than can the Aryan Model.”⁸

VOLUME 1 OF THE *Black Athena* SERIES is dedicated to Bernal’s father, John Desmond Bernal, a historian of science, “who taught me that things fit together, interestingly,” and it is in fitting things together, interestingly, that Bernal’s book represents a tour de force. Some critics argue that things are fitted together too interestingly, with insufficient regard for their context (source criticism) or chronology;⁹ that Bernal, to use his own terms, is too much a “lumper” (“broad synthesizer favored by lay opinion”), too little a “splitter” (“narrow specialist, favored by professionals”).¹⁰ Few, however, can deny the passion, energy, and labor informing Bernal’s ambitious synthesis of evidence on Greek origins from sources ranging from Greek prehistory to contemporary historiography.

Volume 1 is as rich in ideas as it is in detailed evidence, and this factual and conceptual richness is its major virtue: this is a book that offers as much to think with as to think about. While, as critics are quick to point out, Bernal is not the first to acknowledge the debt of Bronze Age Greece to Egypt and the Levant, he is the first to integrate fully his survey of theories on Greek origins into a sociology of knowledge that explains the shift of theories as the result of extrinsic—largely racist—social and political forces.¹¹ Post Bernal, it has become difficult to turn uncritically to traditional scholarship on Greek prehistory without questioning the author’s bias toward an Aryan or Ancient Model, and this is the book’s major achievement. Whether and in what way Volume 1 will have a lasting effect on the direction of classical scholarship and teaching remains to be seen. Optimally, Bernal’s work would spur the specialists toward renewed efforts to study Greece within a Mediterranean context, to examine further the mechanics and processes of cultural interchange, to sharpen the definitions of the ways in which the ancient Greeks were both like and unlike their neighbors. At the same time, his work could challenge classicists—archaeologists, historians, literary critics, and lin-

⁸ Bernal, *Black Athena*, 2: 3.

⁹ John Baines, for example, in his review of Vol. 2, “Was Civilization Made in Africa?” *New York Times Book Review* (August 11, 1991): 12–13, criticizes Bernal’s reliance on evidence from widely different periods in his arguments for Egyptian conquests in Anatolia and the Aegean. The conjunction, argues Baines, could be “pure coincidence”; p. 13. On source criticism, see discussion below.

¹⁰ Bernal, *Black Athena*, 1: 417. Compare Frank M. Turner, “Martin Bernal’s *Black Athena*: A Dissent,” in *Challenge of “Black Athena,”* 109, who calls for “a more particularistic approach which seeks to understand the twists and turns of the lace before asserting the character of a larger pattern,” with Bernal’s response (“*Black Athena* and the APA,” 26): “It is the old difference between ‘lumpers’ and ‘splitters.’ I am a congenital lumper: I like putting things together and I am crude. I am more concerned with overall form than I am with specific content. Clearly, any good history needs both tendencies.” On the terms, see J. H. Hexter, *On Historians: Reappraisals of Some of the Makers of Modern History* (Cambridge, Mass., 1979), 241–43. For an eminently sensible discussion of these issues, see Finley, “Generalizations in Ancient History.”

¹¹ Martin West (briefly cited in Bernal, *Black Athena*, 1: 413; 2: 558 n. 125), Walter Burkert (extensively cited in 2: 165–66 *et passim*), and Chester Starr are the omissions most frequently noted; see Bracht Branham, “Hellenomania,” *Liverpool Classical Monthly*, 14 (April 1989): 57–58; Poliakov, “Roll over Aristotle,” 26 n. 3. Bernal himself particularly acknowledges his debt to Cyrus Gordon (whose works include *The Common Background of Greek and Hebrew Civilizations*) and Michael Astour (*Hellenosemitica*); *Black Athena*, 1: 36–37, 416–22; Bernal, “*Black Athena* and the APA,” 18–20.

guists—both to transcend their traditional intradisciplinary barriers and to breach the current disciplinary walls that divide classics from Near Eastern studies and Egyptology.¹² The worst-case scenario—already being realized—would be for Bernal's work further to fuel the shouting match over "who stole what from whom." One thing, however, is certain: right or wrong or somewhere in between, *Black Athena* has evoked and will continue to evoke a response from students of Mediterranean antiquity. Despite the wishes of some scholars, Bernal's book will not simply go away.

Volume 1 appeared in 1987, but the professional verdict is still not yet in on whether there are sufficient substantive errors in fact or emphasis to undermine the general credibility of Bernal's arguments. On one count, Bernal today himself admits error: although, according to his own sociology of knowledge, Bernal's charges of a massive cover-up by the "classics establishment" should have provoked a rapid and outraged response by professional classicists and ancient historians, this response has been slow to come.¹³ But Bernal should beware of relying on the very *argumentum ex silentio* that he criticizes in his book.¹⁴ Classicists surely have felt incompetent to judge Bernal's account of European historiography and intellectual history, such as his explication of the connections between the nineteenth-century British penetration of the Chinese market with Lancashire cottons and Indian opium, the transformation of the Western image of China, the linguistic demotion of Chinese, and the fall in the linguistic position of Egypt and China, which was paralleled by one in their anatomical and racial status.¹⁵ By the same token, modern European historians may have felt daunted by a work that argues about the Greek Bronze Age and bristles with transliterated hieroglyphs. Substantive criticism from classicists will predictably focus on Volume 2 and future volumes. Yet, even here, as John Baines observes, "because [Bernal] encompasses material from a wide range of disciplines and because much of his discussion of that material is very specialized, hardly anyone will be competent to assess all of it."¹⁶ The enormous chronological and disciplinary sweep of the work ultimately means that it requires a committee to review Bernal properly.

Furthermore, the fact that Bernal has put racism at the forefront of the scholarly equation may inhibit many who differ with him from speaking out, lest they, too, be labeled racists. Some critics have taken issue with what they see as Bernal's "obsession" with racism in his sociology of European historiography and its legacy to contemporary classical scholarship and teaching,¹⁷ but in my opinion

¹² See M. Myerowitz Levine, "Multiculturalism and the Classics," *Arethusa*, 25 (1992): 215–20.

¹³ Bernal, "Black Athena and the APA," 17–20; Bernal, *Black Athena*, 2: xviii–xix. Vol. 1 was the subject of a presidential panel at the annual American Philological Association Meetings in 1989, published with additional papers as *Challenge of "Black Athena."*

¹⁴ Bernal, *Black Athena*, 1: 9–10.

¹⁵ Bernal, *Black Athena*, 1: 237–38.

¹⁶ Baines, "Was Civilization Made in Africa?" 12; compare John Ray, "Levant Ascendant," *Times Literary Supplement* (October 18, 1991): 3.

¹⁷ R. B. Lloyd, *Choice*, 25 (June 1988): 1547, is troubled by Bernal's "obsession with a grand anti-Semitic conspiracy"; compare Muhly, "Black Athena versus Traditional Scholarship," 87–88, on Bernal's treatment of Julius Beloch; Turner, "Martin Bernal's *Black Athena*," 101–09, on methodological deficiencies in Bernal's characterization of nineteenth-century British classical scholarship; with Bernal's response, "Black Athena and the APA," 26–30. Poliakov, "Roll over Aristotle," 16, objects to Bernal's representation of de Tocqueville as a sympathizer with Gobineau's racist theories;

not enough specific weak links have yet been exposed to break the chain of Bernal's arguments regarding the role of racism in "the fabrication of ancient Greece from 1785 to 1985." Volume 1, in this respect, remains still competitive, still plausible.

Classicists have thus far focused on the substance and methodology of Bernal's reconstruction of the Ancient Model and his arguments for his own Revised Ancient Model, while working under the handicap that Volume 1 reserved full argumentation of this aspect of Bernal's case for forthcoming volumes. Much of this criticism seems to me to skirt the real problem of the work, while characterizing as weak points its true strengths.

Bernal's work has been described as "nineteenth century," and this is not usually meant as a compliment.¹⁸ Rather, it is enunciated in the tone of an exasperated parent deflating the would-be bad boy by explaining to him that he is not really that bad at all.¹⁹ Critics have been quick to note the paradox: while very much a product of the late twentieth-century American *Zeitgeist*, Bernal's work—in scale (sweeping), style (dense text, a formidable scholarly apparatus), methodology (quirkily positivist, diffusionist), and content (race and origins)—very much resembles the nineteenth-century historical scholarship that it deconstructs. Frank M. Turner, for example, locates Bernal's work within the nineteenth-century scholarly tradition of "engaged, polemical classical scholarship," which saw moral judgment as one part of the historian's task, and within which "the consideration of the ancient world is subordinate to or at least on an equal level with a modern political or religious concern."²⁰ The audacious sweep of Bernal's notion of a generalized Ancient Model runs counter to the specialization and cautious generalizations of twentieth-century classical historiography. Thus Bernal's notion of an Ancient Model constructed from disparate ancient sources, each bearing its own set of interpretative problems, has been criticized as too

Gleaves Whitney, "Is the American Academy Racist?" *University Bookman*, 30 (1990): 4–15, surveys teaching texts currently used in Western Civilization courses and concludes that "Bernal's claim that the Aryan Model has dominated American scholarship into the 1980s through sheer inertia is beginning to look like a straw man indeed"; p. 10.

¹⁸ Jonathan Hall, "Black Athena: A Sheep in Wolf's Clothing?" *Journal of Mediterranean Archaeology*, 3 (1990): 247–54; Stuart Manning, "Frames of Reference for the Past: Some Thoughts on Bernal, Truth and Reality," *Journal of Mediterranean Archaeology*, 3 (1990): 255–74 (with particular reference to archaeology). Critic Martha Malamud (review of Vol. 1, *Criticism*, 31 [1989]: 322) notes the "curiously old-fashioned" nature of Bernal's enterprise, which "uses the techniques traditional to classicists [philology, linguistics, history of classical scholarship] to re-examine the same body of texts and archaeological evidence that classicists have always studied to carve out radically different historical models." This approach then could conceivably fail to satisfy either traditional philologists (who would prefer more traditional conclusions) or new critical theorists (who would prefer more currently fashionable methodologies). Malamud perceptively remarks, "It is conceivable that *Black Athena* will become notorious, yet fail to be influential, precisely because it provides the warring factions in the field with a common enemy." If this turns out to be the case, Bernal's nonconformity, his perceived failure to fit coherently into any recognizable scholarly posture, new or old, is a virtue masked as vice, the problem not of the book but of its critics.

¹⁹ In the conclusion to *Black Athena*, 2 (522–27), somewhat with the air of an *enfant terrible* thumbing his nose at his elders, Bernal himself catalogues and explains his "outrages" lest, buried under the mass of traditional late nineteenth through early twentieth-century scholarship, they escape an uncritical reader's notice.

²⁰ Turner, "Martin Bernal's *Black Athena*," 100.

general and so oversimplified that it is unhelpful: "The application of a model is meant to simplify evidence, but at what point does it begin to oversimplify?"²¹

Methodological criticisms of Volume 1 often take a peculiarly involuted path. Bernal has been criticized as too credulous of the ancient sources, particularly Athenian sources, which, after the Persian Wars, were themselves informed by intensified cultural chauvinism. His Ancient Model, it is claimed, is constructed with too little regard for the ideological context of these sources—especially Herodotus—which must be read first as social constructs created and transmitted primarily to validate contemporary reality.²² In the same vein, Bernal's reading of myths of Egyptian and Semitic colonization has been characterized as rooted in a naïve, nineteenth-century view of myth as "coded history," which fails to take into account more sophisticated twentieth-century approaches to myth.²³

To confuse the issue further, Volume 1 links the development during the eighteenth and nineteenth centuries of source criticism in classical historiography to the rise of the Aryan Model, describing it as an important scholarly tool used to discredit those ancient sources that accepted Egyptian and Semitic influences.²⁴ But Bernal himself obviously uses source criticism not only on his nineteenth-century sources but on his ancient sources as well. Herodotus, for example, who despite national prejudice includes traditions of Egyptian colonization in Greece is regarded as a good source, while Thucydides, who fails to mention these legends, "of which he was certainly aware," must have been motivated by national prejudice.²⁵ In other words, Bernal rejects the total skepticism of some of his critics regarding the historical usefulness of the ancient mythic sources and selectively argues (in respectable nineteenth-century fashion) for the reliability of those sources that support his argument. In fact, Bernal, his critics, and many of the nineteenth-century historians whom he deconstructs are all using source criticism. They simply disagree on which if any sources are to be believed.

But Bernal must go beyond arguing for the credibility of sources now often dismissed as ideologically (and therefore historically) unreliable, to take on the argument that forms a cornerstone of the Aryan Model's rejection of ancient sources on Greek prehistory. The ancient sources "at best . . . are dubious sources

²¹ Branham, "Hellenomania," 59. On Bernal's notion of "models" and Kuhn's paradigms and disciplinary matrices, see Bernal, *Black Athena*, 1: 3; 2: 11–12, with criticisms of J. Hall, "Black Athena: A Sheep in Wolf's Clothing?"

²² Tamara Green, "Black Athena and Classical Historiography: Other Approaches, Other Views," in *Challenge of "Black Athena"*, 55–65, with Bernal's rebuttal, "Black Athena and the APA," 23–25; compare Morris, "Daidalos and Kadmos," 50–51 (on Athenian historical revisionism after the Persian Wars); Poliakoff, "Roll over Aristotle," 14–15. Bernal's argument, echoing William Mitford's in *History of Greece*, points out that the intense cultural chauvinism of the Greek sources after the fifth century B.C.E. "makes it still more remarkable that legends of settlement and profound cultural contacts should have been preserved"; Bernal, "Black Athena and the APA," 22–23; compare Bernal, *Black Athena*, 1: 100–01.

²³ Branham, "Hellenomania," 59; J. Hall, "Black Athena: A Sheep in Wolf's Clothing?" 251; most fully, E. Hall with Bernal's rebuttal, "When Is a Myth Not a Myth?" and "Response to Edith Hall." For Bernal, "the fact that myths have intricate and complicated structures and that some of these complexities can be explained interestingly by modern scholars has no bearing on the question of their having or not having any historicity"; Bernal, "Response to Edith Hall," 204.

²⁴ Bernal, *Black Athena*, 1: 217–18, 221, 302 *et passim*.

²⁵ Bernal, *Black Athena*, 1: 23, 98–103.

for the history of the second millennium B.C.E., which is Bernal's focus,"²⁶ if and only if the classical Greeks are assumed to have known even less about their own prehistory than contemporary archaeologists and historians do. Received wisdom, at least until recently, has been that the breakup of the Mycenaean world in the twelfth century B.C.E. was followed by a "Dark Age" (dark at least to historians), which hangs like an impenetrable curtain between the Greek Bronze and Archaic Ages. The break in the historical record—fast being filled in by Aegean archaeology—was assumed to indicate a radical cultural break. Thus, at the extreme, it could be argued that "the pattern of civilization . . . which we call 'Greek' and which has directly influenced all subsequent Western history, was evolved only in the centuries between 1100 and 650 B.C."²⁷

Bernal uses ancient traditions in tandem with many other types of evidence (archaeological, for instance) that cast contemporary light on the Dark Age. But he knows he must confront this period of cultural discontinuity in Greece, both in order to support the reliability of his classical sources for the earlier period and to prove the formative effect of Egyptian and Near Eastern Bronze Age contacts on Classical Greek civilization (and, through these, on Western culture). Not only does Bernal argue that his "late" ancient sources had a "feel" for the societies they describe in a way that modern scholars cannot equal (certainly not the framers of the Aryan Model, who worked before twentieth-century archaeological data and techniques), he also makes a valiant attempt to shorten the period of "darkness" by moving the Ancient Model back from Classical to Archaic (776–500 B.C.E.) and Geometric Greece (950–776 B.C.E.); arguing for religious continuity on the basis of the Linear B tablets; and arguing, despite the lack of any surviving Greek inscription before the eighth century B.C.E., for an early date for the transmission of the West Semitic alphabet to Greece (before 1400 B.C.E.).²⁸ This last argument would, if accepted, provide writing as a vehicle for cultural continuity through the Dark Age. Confirmation or refutation of Bernal's arguments for significant cultural permeability through the curtain of the Dark Age must come from

²⁶ Branham, "Hellenomania," 59.

²⁷ Chester G. Starr, *The Origins of Greek Civilization 1100–650 B.C.* (New York, 1961), 5. See also Rhys Carpenter, *Discontinuity in Greek Civilization* (New York, 1968). At the same time, there has long been awareness on the part of many that cultural continuity from the Bronze through the Dark to the Archaic Age and beyond is not to be measured in terms of the impoverished material remains of the Dark Age. Decipherment of the Linear B tablets revealed that divinities of Archaic Greece were worshiped by Bronze Age ancestors; John Chadwick, *The Mycenaean World* (Cambridge, 1976), 84–101, adding weight to Martin P. Nilsson's argument that classical Greek religion developed from the Mycenaean; Nilsson, *The Mycenaean Origin of Greek Mythology* (Berkeley, Calif., 1932, 1983). Widespread scholarly acceptance of Milman Parry's insights into the oral nature of Homeric poetry removed the obstacle provided by presumed Dark Age illiteracy to the idea of Greek epic poetry reaching back to Mycenaean heroes and settings. Analyses of Mycenaean art have emphasized the continuity of artistic techniques and design between this era and the era of "Greek" art proper; Roland Hampe and Erika Simon, *The Birth of Greek Art from the Mycenaean to Archaic Period* (Oxford, 1981).

²⁸ On modern scholars, see *Black Athena*, 2: 4; compare Bernal, "Response to Edith Hall," 209–10. On the period of darkness, see *Black Athena*, 2: 4–5; 2: 7–9; 1: 393–99, 432. See also Martin Bernal, "On the Transmission of the Alphabet into the Aegean before 1400 B.C.," *Bulletin of the American Schools of Oriental Research*, 267 (1987): 1–19; *Cadmean Letters: The Transmission of the Alphabet to the Aegean and Further West before 1400 B.C.* (Winona Lake, Ind., 1991); compare Morris, "Daidalos and Kadmos," 44–45.

archaeologists and epigraphers, present and future.²⁹ In this connection, it deserves note that the very notion of a Dark Age often dies harder for historians than for archaeologists, perhaps because fine and strict distinctions may look neater in the library than they do in the field. Contact with messy *materia* may make archaeologists more sensitive to continuity than to discontinuity.

Finally, there is Bernal's championing of diffusionism, characterized by many as "old fashioned."³⁰ In Bernal's modified diffusionist model—grossly oversimplified as the "billiard ball model"—of cultural transmission, he joins the company of nineteenth-century scholars and parts with now popular isolationist theories of indigenous development. The isolationist explanation for cultural development, championed by the British archaeologist of prehistory Colin Renfrew, emerged during the first half of the twentieth century partly in reaction to Western cultural colonialism, which diffusionist theories supported.³¹ Here again, we see the very superficial paradox of Bernal's deconstruction of nineteenth-century scholarly racism with nineteenth-century methodological assumptions—assumptions that, though not necessarily wrong, were often used uncritically to support colonialist racism. Isolationism, however, can cut both ways. Like the ancient myths of autochthony, which bolstered fifth-century Athenian chauvinism, isolationism with its emphasis on indigenous development can support "Aryan" cultural chauvinism. Conversely, Bernal's diffusionist theory posits cultural influences flowing from East (the Levant) and South (Egypt) to West and North (Greece), rather than the flow from North to South and East assumed by diffusionist supporters of the Aryan Model.

Diffusionism, too, can be as politicized today as it was in the nineteenth century. Misapplied, it can be the match that ignites the tinderbox of neo-racist theories of origins. It can become the "who gave what to whom" that leads to the "who took what from whom," which culminates in the "who stole what from whom"—phrases within which much of the discourse on race and origins within the context of the United States in the 1990s is now framed.³² Somewhere between the extremes of diffusionism and isolationism is a middle ground that admits the possibility of indigenous development and cultural borrowing and, in the latter case, places equal emphasis on transformation, the idea that "cultural reception rarely operates mechanically; the receiving culture is at least as actively engaged

²⁹ Morris, "Daidalos and Kadmos," 48, calls for the elimination of "the deliberate distance kept between the second millennium and the culture of classical Greece," citing the concept of a Dark Age as a major modern factor keeping the Bronze and Iron Ages artificially apart; she provides many instances of significant cultural continuity.

³⁰ Branham, "Hellenomania," 59; compare Baines, "Was Civilization Made in Africa?" 13; Muhly, "Where the Greeks Got Their Gifts," 3; Edmund Leach, "Aryan Warlords in Their Chariots," *London Review of Books* (April 2, 1987): 11; Sarah Morris, "Greece and the Levant: A Response to Martin Bernal's *Black Athena*," *Journal of Mediterranean Archaeology*, 3 (1990): 63; J. Hall, "Black Athena: A Sheep in Wolf's Clothing?" 248–49.

³¹ Bernal, *Black Athena*, 1: 407; 2: 64–67.

³² There is too much macho war imagery both in Bernal's use of the term "ammunition" (*Black Athena*, 1: 359, 412) and in the language of some of his more engaged readers. Greg Tate praises Bernal for putting "a heap more ammo in the reading room"; Tate, "History: The Colorized Version, or Everything You Learned in School Was Wrong," *Village Voice*, 28 (March 1989): 50.

in interpretation, evaluation and adaptation as the donor culture is in the production and dissemination of its cultural wares.”³³

In emphasizing the Greek debt to Egypt and the Levant, Bernal has been accused of neglecting transformation, of bending “the stick so far in the opposite direction that it is the Phoenicians and Egyptians who are responsible for everything.”³⁴ Still, cultural chauvinism—even on the level of the transformation of acknowledged borrowings—can bend the stick back too far in the former direction. Cicero grudgingly admits Rome’s cultural debt to Greece while insisting, “Our people have shown more wisdom than the Greeks, either in making independent discoveries for themselves, or else in improving upon whatever they had received from the Greeks—at least in those matters that they judged worthy of their efforts.” The author of the *Epinomis* attributes Greek astronomy to Egyptians and Babylonians but claims that “whatsoever the Greeks take from the Barbarians, they finally carry it to a higher perfection.”³⁵ The perfect instrument for measuring both diffusion and transformation (including linguistic diffusion and transformation) has yet to be found—a *fortiori* diffusion and transformation in the shadows of prehistory.

DIFFUSIONISM SERVES AS A RED HERRING for the more problematic issue of Volume I’s very nineteenth-century focus on origins and race. This issue constitutes the dangerous part of this interesting and dangerous book.

“Who were the Greeks?” Do we define “Greek” by origins or achievements? Definition by origins is alien to the liberal humanist tradition, which constructs identity not from birth but from achievement. (Recall that the ode to Athenian excellences in Pericles’ funeral oration has not one word on origins and many about accomplishments. In the high tide of life, past origins pale in the face of the splendid present.³⁶) “What was ‘classical’ about classical Greece, after all, was not its language or its religion—Athena white, black or brown—but its politics, philosophy and art. To set Greek history in its proper Near Eastern context is a necessary antidote to all miraculism; but to resolve it into that context would be a no less implausible reductionism,” argues Perry Anderson.³⁷ Bernal’s analysis of scholarly racism inevitably reopens the nineteenth-century Pandora’s box of racial hatred, just as his focus on Greek origins agrees with the late twentieth-century American notions of ethnicity, which abandon earlier visions of the “melting pot” in which past origins merge into a common present and future.³⁸ Bernal, to some

³³ Branham, “Hellenomania,” 59; compare, J. Hall, “*Black Athena: A Sheep in Wolf’s Clothing?*” 250–51, on criteria for distinguishing culturally meaningful linguistic and mythic diffusions.

³⁴ Constantine Giannaris, “Rocking the Cradle,” *New Statesman* (July 10, 1987): 31.

³⁵ Cicero, *Tusculan Disputations* 1.1; *Epinomis*, 987D9–E1.

³⁶ Thucydides 2.35–46.

³⁷ Perry Anderson, “The Myth of Hellenism,” *The Guardian* (March 13, 1987): 14; compare David Gress, “The Case against Martin Bernal,” *The New Criterion* (December 1989); Branham, “Hellenomania,” 60. Muhly, for example, “*Black Athena* versus Traditional Scholarship,” 87, charges that Bernal’s equation of democracy in Phoenicia with fifth-century Athenian democracy on the basis of origins exhibits a “total misunderstanding of ancient Greece and Classical Greek civilization.”

³⁸ See Arthur M. Schlesinger, Jr., *The Disuniting of America* (Knoxville, Tenn., 1991); compare Diane Ravitch, “Multiculturalism: E. Pluribus Plures,” *American Scholar* (Summer 1990): 337–54, with

degree, shares this “melting pot” vision, applying it to antiquity when attributing the Greek “miracle” to an extraordinarily productive mix of origins: “I’m not trying to challenge the centrality of Greece to the European tradition, but I do challenge the nature of Greece itself. Greek culture is so exciting because it represents a mix of the native Balkan-Indo-European-speaking population with Egyptian and western Semitic populations. The linguistic and cultural mix was extraordinarily productive in cultural terms.”³⁹

But origins—not accomplishments—are Bernal’s focus, and this leads to the abuse of his history—by both Bernal and his readers. Bernal overemphasizes the influence of racial origins on scholarship; he seems to take it as given that the ethnic group you were born in—Jewish, WASP, African American—greatly determines both what you claim as “objective conclusions” and your reception or rejection (including Bernal’s own reception) by the academy.⁴⁰ What begins as a healthy corrective to the chimera of absolute objectivity in scholarship thus leads to an equally misleading representation of the scholarly enterprise as a cover for racial and ethnic turf wars. As I write these words, I vividly recall what happened when Mediterranean archaeologist James Muhly rejected the politicization of geography in the use of the term “Kemet” for Egypt at the Temple University Conference on *Black Athena*: “I try to understand history as it is, not as I would like it to be. If we speak of Kemet not Egypt, then we must refer to Deutschland not Germany, to Hellas not Greece, Helvetia not Switzerland, München not Munich.”⁴¹ At this, the audience booed and groaned. Such disinterest is no longer believed to be possible. Ergo, Muhly was assumed at best to be a naïf, at worst a hypocrite.

As for Bernal’s readership, his focus on origins and race—particularly the identification of Egyptians as racially black and the location of the origins of Greek civilization in Egypt—has been received with keen interest by African Americans, long denied knowledge of their own origins in Africa as the result of their enforced physical uprooting from Africa and enslavement in America. For African Americans, exile and enslavement meant the disruption of their own historical traditions and the omission and distortion of their history in American and European historiography. Furthermore, it is precisely blacks in the United States who in the context of virulent color prejudice have suffered most from racism that defines and ranks identity exclusively by the biological determinant of racial origins. For this population, Bernal’s work adds to an ever-growing body of popular and scholarly literature that attempts to reconnect black Americans with

essays by Molefi Kete Asante and Ravitch in “Multiculturalism: An Exchange,” *American Scholar* (Spring 1991): 267–76.

³⁹ Norm Allen, “*Black Athena*: An Interview with Martin Bernal,” *Free Inquiry*, 10 (Spring 1990): 18.

⁴⁰ Compare Ray, “Egyptian Perspective,” 79; Poliakov, “Roll over Aristotle,” 16–17; Malamud, *Criticism*, 322; Baines, “Was Civilization Made in Africa?” 12, and references above, n. 17. For Bernal on origins (his own and others’) as a basis for academic acceptance, see “*Black Athena* and the APA,” 19–20; Allen, “*Black Athena*: An Interview,” 21. The *reductio ad absurdum* of this phenomenon is Bernal’s characterization of Frank M. Snowden, Jr.—one of the most outspoken critics of the equation of the ancient Egyptians with modern blacks—as conforming to “white scholarship”; Bernal, *Black Athena*, 1: 436. The label “white” as crudely applied to Snowden by his radical Afrocentrist opponents transforms Snowden (who is black) from black to white solely on the basis of his scholarly conclusions.

⁴¹ Muhly, “Is There Evidence for Egyptian Colonization in Central Greece?” Tape 4, side B.

their African origins by transforming ancient Egypt (about which much information survives) into a metonym for ancient Africa (about which relatively less is known) and by claiming Egypt as the precursor of Greek civilization and, through the Greeks, of Western civilization.⁴² At the same time, the Egyptians themselves, now identified as racially “black,” become a metaphor for all who today define themselves as black. To many in this community, the glorious past of black Egypt and Egypt’s contributions to Western (“European”) civilization become a charter of great past achievements and send a mythic message, anchored in biological determinism, that asserts future great cultural achievements to be a continuation of the past rather than a radical break with it.⁴³

But, in the quest for origins (compare the myths of Amazons for radical feminists), history has its limits. In a discussion of the sociology of black historical knowledge, Orlando Patterson has cautioned:

It is sometimes possible to ask too much of history. If, under our present circumstances, we cannot achieve a status which, by itself, ensures our pride, then no amount of wishful looking back will help us. All we should do then is to make of our past the sand in which we hide our heads. The unemployed Roman pauper has no more pride than his despairing Harlem counterpart, even if he makes his bed each night beneath the walls of the Colosseum. In the final analysis, a stark truth confronts us: that the present gives meaning to the past as much as the past informs the living.⁴⁴

The quest for past origins, if pursued at the expense of present achievement, can become a fatalistic—and fruitless—diversion in an American society that attempts to locate identity (“who you are”) in the present (“what you do”) rather than the past (“who you were born”). To keep the hyphen in “African-American,” as Henry Louis Gates, Jr., remarks, is to focus on “the very complexity of being of African descent in the world” rather than to direct all efforts “to rediscover a lost cultural identity—or invent one that never quite existed.”⁴⁵ To put it another way, there is nothing wrong with using a historical kernel to construct a mythic history

⁴² See Molefi Kete Asante, *Kemet, Afrocentricity, and Knowledge* (Trenton, N.J., 1990); Cheikh Anta Diop, *Civilization or Barbarism: An Authentic Anthropology*, Yaa-Lengi Meema Ngemi, trans., Harold J. Salemson and Marjolijn de Jager, eds. (Brooklyn, N.Y., 1991); Cheikh Anta Diop, *The African Origin of Civilization: Myth or Reality*, Mercer Cook, trans. (Westport, Conn., 1974); Cheikh Anta Diop, *Precolonial Black Africa: A Comparative Study of the Political and Social Systems of Europe and Black Africa, from Antiquity to the Formation of Modern States*, Harold J. Salemson, trans. (Westport, 1987); George G. M. James, *Stolen Legacy: The Greeks Were Not the Authors of Greek Philosophy, but the People of North Africa, Commonly Called the Egyptians* (1954; rpt. edn., New York, 1989); Eloise McKinney Johnson, “Who Homer Really Was,” *College Language Association Journal*, 22 (1978): 54–62; Yosef ben-Jochannan, *Black Man of the Nile (Contributions to European Civilization and Thought)* (New York, 1970); Yosef ben-Jochannan, *Africa, Mother of Western Civilization* (Baltimore, Md., 1988); Yosef ben-Jochannan, *Black Man of the Nile and His Family* (Baltimore, 1989); Chancellor Williams, *The Destruction of Black Civilization: Great Issues of a Race from 4500 B.C. to 2000 A.D.* (1971; rpt. edn., Chicago, 1987).

⁴³ The biological determinism is especially evident in the middle term of the argument (made by some extremists), which sees the ancient wisdom of the Egyptians rooted in the biological makeup of the race and specifically connected with the functions of the pineal gland; see Richard King, *African Origin of Biological Psychiatry* (Germantown, Tenn., 1990); compare Bernard Ortiz de Montellano, “Multicultural Pseudoscience: Spreading Scientific Illiteracy among Minorities—Part 1,” *Sceptical Inquirer*, 16 (Fall 1991): 46–50; “Afrocentric Creationism,” *Creation/Evolution*, 29 (Winter 1991–92): 1–8, with sources cited in bibliography.

⁴⁴ Orlando Patterson, “Rethinking Black History,” *Harvard Educational Review*, 41 (August 1971): 315.

⁴⁵ Henry Louis Gates, “Beware of the New Pharaohs,” *Newsweek*, 118 (September 25, 1991): 47.

of a glorious past; others—Aryans, Jews, Greeks, and Romans—have certainly used “history” as what Bernal calls an “ego massage.”⁴⁶ As with all myths, the problem lies not so much in the construction of such a myth as in the use to which it is put. Too often, in practice, radical Afrocentrists use the mythic history of an Edenic black Egypt, mother of all Western arts and sciences, not to inspire present achievement by changing the “mentality of the Black people . . . from an inferiority complex, to the realization and consciousness of their equality with all the other great peoples of the world, who have built great civilizations,” but to frame that story of past glories as a “stolen legacy” in a language of violence, rape, and despoilment that is in itself counterproductive and that encodes the modern American black experience of enslavement better than any ancient realities of cultural transmission.⁴⁷ Thus the story is told in terms that channel contemporary frustrations and anger away from their proper objects toward imagined despoilment in classical antiquity. Moreover, at the time of this particular telling, the myth makers do not possess the authority of the bard or griot in traditional society. Rather, as twentieth-century intellectuals in an atraditional society, they require scholarly authorization by the modern conventions of critical historiography “to prove” that the story is “true.” Less self-conscious myth makers of the past did not have to deal with this problem, although, as Bernal has shown, nineteenth-century “Aryan scholarship” provides an excellent model for how it could be done.

Radical Afrocentrists, who constitute one of Bernal’s most receptive audiences, misuse his arguments to buttress claims for the irredeemable unreliability of “European” historiography, for the identification of the Egyptians as racially black, and for the location of Egypt as the sole source of Greek and thus Western civilization. (The Semitic part of Bernal’s cultural equation—which, in fact, occupies the major part of Volume 1—is conspicuous by its absence from these claims.) At the same time, Afrocentrists criticize him for his Eurocentric focus, which privileges Greece over Africa as the center of world history. Susantha Goonatilake, for example, characterizes Volume 1 as “both a partial liberation as well as a partial prison. It is a liberation in the sense that it dismantles a racist and imperialist vision which had been constructed on very little evidence. It is a prison because Martin Bernal still holds implicitly to a broad Eurocentric vision.”⁴⁸

THE WHEELS OF SCHOLARSHIP IN THE HUMANITIES are notorious for turning overcautiously and slowly. The title page of one recent prestigious classics journal offers a menu of scholarly discussions of Pindar, Athenian trierarchies and

⁴⁶ Bernal, “Response” at the symposium “Challenging Tradition: Cultural Interaction in Antiquity and Bernal’s *Black Athena*,” Tape 7, side A.

⁴⁷ James, *Stolen Legacy*, 153.

⁴⁸ Susantha Goonatilake, “The Son, the Father, and the Holy Ghosts,” *Economic and Political Weekly* (August 5, 1989): 1769. Compare the critiques of Maulana Karenga, “The Contested Terrain of Ancient Egypt: Diop, Bernal and Paradigms in Africana Studies,” and Molefi K. Asante, “Response,” at “Challenging Tradition: Cultural Interaction in Antiquity and Bernal’s *Black Athena*,” Tape 5, sides A and B.

assemblies, the *palliat togata*, the Scipionic Circle, and the poetic ego of Horace. Nary a mention of the “red-hot debate over what role Egypt played in shaping the glory that was Greece” leading to the “incendiary question: was Egypt ‘black’?” in which, according to *Newsweek*, “classics departments from Oxford to Harvard are embroiled.”⁴⁹ These questions, of course, represent the most immediate and popular political implications of Bernal’s treatment of Greek origins in Volume 1.

No longer a book but a phenomenon, *Black Athena* is supposed to have left a shellshocked academy recovering from a war that many classicists never realized was going on, much less that they had lost. Nor is this general insensitivity to “current events” confined to the “classics establishment.” Historians, too, have been lamentably slow to take a stand on the issues raised by Bernal’s book and then taken up in the popular press. I explain this neglect partly by anachronistic and counterproductive divisions between departments of classics and history that result in ancient history being tossed back and forth like a football neither team is obliged to catch; partly, as suggested above, by fear of accusations of racism for suggestions that Bernal’s arguments or the claims of the radical Afrocentrists may be flawed; partly by an elitist and separatist attitude that marginalizes black scholarship and the arguments of Afrocentrists in the fallacious assumption that these are not and will never be part of the academic mainstream: “Let them believe that Egyptians were black, that the Greeks stole their knowledge from Egypt—it doesn’t really matter.” Whatever the reason, the wheels of the historians and classicists move more slowly than the history that is the object of their study. Bernal himself is curiously selective in his response to racism in history, deserving high marks for his impassioned and far-ranging reconstruction of eighteenth and nineteenth-century European intellectual history but a shockingly low grade for his reading of the contemporary American *Zeitgeist*.

The aspect of Bernal’s case that has (thus far) generated the most heat is the title of the work, with its implication that the Egyptians were black.⁵⁰ One can easily sympathize both with the annoyance of nonwhite people at what may seem to be peculiarly perverse pedantry over the application of the term “black” to ancient Egyptians and with the frustrated exhaustion of the few scholars who have dared participate in the often circular debate over the definition of the word “black.” The term, perhaps once useful as a “classificatory label” enabling historical discourse, is by now “so abused in American racial politics that scholarly precision is unlikely to have much impact on public controversy.”⁵¹

Frank M. Snowden, Jr., emeritus professor of classics at Howard University and long-time pioneer in scholarship on blacks in classical antiquity, argues that *Black Athena* is a misnomer for “Egyptian Athena.” To Snowden, Bernal’s equation of Egyptians with Negroes or blacks is contradicted not only by the Greek historian

⁴⁹ “Out of Egypt, Greece,” *Newsweek*, 118 (September 23, 1991): 49. See the thoughtful essay of classicist Mary Lefkowitz, “Not Out of Africa,” *New Republic*, 206 (January 10, 1992): 29–36; and letters by Bernal and Lefkowitz, *New Republic*, 206 (March 9, 1992): 4–5. The fact that this treatment of the subject appeared in a popular journal further supports my point here.

⁵⁰ See, for example, the round of outraged letters generated by an advertisement for a white Nefertiti doll in *Biblical Archaeology Review*, 15.1 (1989): 3–4; 15.3 (1989): 18; 15.5 (1989): 8–12.

⁵¹ Jane F. Gardner, “The Debate on *Black Athena*,” *Classical Review*, 41 (1991): 167; compare Finley on generalizations implicit in anachronistic terminology; *Use and Abuse of History*, 62–64.

Herodotus but by other classical as well as Egyptian evidence. If we are attempting—anachronistically—to find classical equivalents for the concept of black or Negro as generally understood today, argues Snowden, it is strictly to the people described in classical sources as Ethiopian that we must turn. Furthermore, the ancient sense of a collective “us” versus “them” was a distinction based mainly on cultural phenomena (language, customs) rather than skin color. To impute contemporary color-based racial consciousness to classical sources, maintains Snowden, is to misread the ancient texts through the prism of modern notions of race based on skin color.⁵²

Bernal himself would partially agree. “I am now convinced that the title of my work should have been African Athena,” he states, but only because “black” to his mind has been understood too narrowly as representing purely West African physical types rather than the whole spectrum of “nonwhite” skin hues and physical types that grow darker and more Negroid the farther south or up the Nile one goes.⁵³ As Egyptologist Frank Yurco has observed, “many people today who consider themselves Afro-American or ‘black,’ particularly those of mixed racial ancestry, would have no trouble finding physical types resembling their own in contemporary Egypt.”⁵⁴

Snowden’s equation of modern “black” with ancient “Ethiopian” (*Aithiops*, *Aethiops*) derives from the classical Greek and Roman sources for whom neither Egyptian nor African is necessarily a synonym for the term “black” as popularly understood today. Modern physical anthropologists—while avoiding the term “black” as imprecise—recognize the Egyptians as one end of a continuum of interrelated populations stretching across the African continent whose color intensity varies in rough correlation to distance from the equator. Anthropological discussion of the identity of the ancient Egyptians is complicated by the fact that the southern Egyptian population from the region that gave birth to Egyptian civilization (between Cairo and Aswan) changed over time. According to some recent studies, the earliest pre-dynastic Egyptians were both less variable and closer to tropical African populations than later populations. Furthermore, over

⁵² See Herodotus 2.104, a passage that has long served as the *locus classicus* for claims that the Greeks identified the Egyptians as black; compare Bernal, *Black Athena*, 1: 242 n. 68. See Frank M. Snowden, Jr., “Bernal’s Blacks, Herodotus, and Other Classical Evidence,” in *Challenge of “Black Athena,”* 83–95; see also Frank M. Snowden, Jr., *Blacks in Antiquity* (Cambridge, Mass., 1970); *Before Color Prejudice: The Ancient View of Blacks* (Cambridge, Mass., 1983).

⁵³ Bernal, “*Black Athena* and the APA,” 31; compare Bernal, *Black Athena*, 1: 241–42. On the archaeological evidence for the color of the Egyptians’ skin, see David O’Conner, “Ancient Egypt and Black Africa—Early Contacts,” *Expedition: The Magazine of Archaeology/Anthropology*, 14 (1971): 2–9; Frank Yurco, “Were the Ancient Egyptians Black or White?” *Biblical Archaeology Review*, 15 (September–October 1989): 24–29; David H. Kelly, “Egyptians and Ethiopians: Color, Race, and Racism,” *Classical Outlook*, 68 (Spring 1991): 77–82.

⁵⁴ Yurco, “Were the Ancient Egyptians Black or White?” 29. See also Yurco’s remarks on the distortion by Afrocentrists of the evidence for links between Egypt and Greece; “Letter to the Editor.” Modern Egyptians themselves have a hard time understanding the American fuss over color. See, for example, *The Washington Post* report (“Egypt Says Ramses II Wasn’t Black,” March 23, 1989, D8) on the controversy surrounding the “Ramses the Great” exhibit in Dallas, Texas, March–August 1989. Egypt’s cultural emissary to the United States responded in bewilderment to the contention made by a group called the Blacology (*sic*) Speaking Committee that Ramses II was black: “This is an Egyptian heritage and an Egyptian civilization 100 percent. We are part of the African continent. We cannot say by any means we are black or white. We are Egyptian, with our culture and traditions and religions.”

time, the southern populations of Egypt grew more variable and exhibited more commonalities with Egyptians from the Delta, who themselves shared more traits with peoples from the Near East from dynastic times on.⁵⁵ Many Afrocentrists conflate the positions of Snowden and the physical anthropologists, using “black” loosely as an umbrella term for “people of color,” defining all Egyptians and all Africans as “black,” while asserting that the terms “Egyptian” or “African” in the classical sources agree with their definition.⁵⁶ But the scholarly debate is eclipsed by the high voltage ideological charge of political controversy, now fueled by Bernal’s “arresting but grossly misleading” title.⁵⁷

Thus it is that now, at a time of national debate over the “Eurocentrism” of humanities curricula and canons, the “blackness” of *Black Athena* stands as the most politically charged issue of the book. The racial identification of the Egyptians as black has become the linchpin of impassioned arguments—heard primarily in Afro-American Studies and among some black students—for a black origin for much of Greek civilization and thus Western culture. At their most extreme and most popular, these claims assert a North African origin for the Pelasgians and identify among others Homer, Socrates, and Euclid as African blacks. At their most extreme, these claims assume the nineteenth-century sounds of a racist Kulturkampf.

While such “charter myths” may be useful to their constituencies, they are also profoundly ironic, for they painfully recall the zealous excess with which nineteenth-century Europe appropriated and remade ancient Greece in its own “Aryan” image—the very events so impressively charted and decried by Bernal. However satisfying to aesthetic or moral symmetries, this contemporary movement is no less frightening or dangerous:

When former victims wrest from the oppressors the bashing stick and bash back, our sense of poetic justice may indeed prompt us to applaud, rather than to denounce bashing, whoever holds the stick . . . Taking pride in origins, whoever does so, courts the tempta-

⁵⁵ I thank anthropologists Alison Brooks (George Washington University) and Shomarka Keita (Howard University) for this note. According to S. O. Y. Keita (private communication), the notion of “Negro” or “black” used to describe a single set of physical characteristics has been superseded by the understanding that the range of variation seen in tropical Africa is not due to intermarriage with people who came from Asia or Europe but rather is to be seen as variants of a common African lineage that has differentiated by microevolutionary forces. The broadfeatured, black-skinned, woolly-haired, so-called “Negro” phenotype is not a Platonic ideal of “Negro” but merely a variant of a classificatory unit more appropriately termed as Tropical African, much as Nordic is merely a variant of a European or “white” type. See S. O. Y. Keita, “Studies of Ancient Crania from Northern Africa,” *American Journal of Physical Anthropology*, 83 (1990): 35–48; and “Further Studies of Ancient Crania from Northern Africa,” 88 (1992); Bruce G. Trigger, “Nubian, Negro, Black, Nilotic?” *Africa in Antiquity: The Arts of Nubia and the Sudan*, Sylvia Hochfield and Elizabeth Riefstahl, eds., vol. 1 (New York, 1978), 27–35; A. Caroline Berry, R. J. Berry, Peter J. Ucko, “Genetical Change in Ancient Egypt,” *Man*, n.s. 2 (1967): 551–68, with additional references in bibliography.

⁵⁶ Jacob Carruthers, for example, “Outside of Academia: Bernal’s Critique of the Black Champions of Ancient Egypt,” at “Challenging Tradition: Cultural Interaction in Antiquity and Bernal’s *Black Athena*,” Tape 1, side B, argues regarding the racial identification of the Egyptians: “Let me just simply assert here and now that the pharaohs, not only the pharaohs, but also the Kemitic people were black. And what [do] I mean by that? That the Kemites were black as the term is generally used today but [also] as I believe the term was used in the time of Herodotus and Aristotle. When used to designate the phenotype associated with what we call the continent of Africa, the term includes a variety of shades ranging from light brown to what we call black.”

⁵⁷ G. W. Bowersock, review of *Black Athena*, vol. 1, *Journal of Interdisciplinary History*, 19 (1989): 490.

tion of making one's sense of worth exclusionary and of founding it on a complacent past that is largely fabricated to suit that end, rather than basing it on a present course of action that is molded to the advantage of the whole community.⁵⁸

Furthermore, the radical Afrocentrists may be helping to fuel the new racism seen today on many college campuses; one recent and compelling analysis sees the resurgence of 1980s racism as a complex but direct response to a "politics of difference," which does not merely emphasize racial identity but lays claim to empowerment and entitlement on the basis of race alone.⁵⁹ Worse still, and in view of the media attention to Political Correctness, frighteningly possible, would be a wrongheaded reaction in the form of a backlash against all critical analysis of historiography, making the work of historians truly impossible. So the wheel swings around, and the paradoxes multiply. Although Bernal's avowed intention is to argue for a more ethnically pluralistic picture of early Greece, *Black Athena* can and is being used to underscore difference rather than similarity, to further separation rather than mingling, to propose otherness rather than sameness; its arguments are being adopted enthusiastically and uncritically by many nonclassics precisely because of its ideological congeniality, impressively enhanced by the academic credentials and broad learning of its author.

Bernal himself, in my view, takes too little regard of the ways in which such things fit together interestingly, but dangerously, today. Caught between his Scylla of Cheikh Anta Diop and his Charybdis of Frank Snowden, he has opted for the former (or been lumped with the former, as in John Baines' and John Ray's joint reviews of the two).⁶⁰ In Volume 1, Bernal noted the "general hostility among them [black scholars] to Semitic culture, especially when it is supposed to have affected Egypt" and expressed the hope that his work might "reconcile these two hostile approaches." But this aspect of Bernal's project, in particular, has not been successful.⁶¹ In his survey of the reception of Volume 1 prefacing Volume 2, Bernal distances himself somewhat from the way in which his ideas are being used: "They have become public property over which I have little control or even influence. This is, of course, quite proper, because the ways in which ideas are received are much more important than their author's original and often convoluted intentions."⁶²

Plato, whom Bernal treats as heavily influenced by Egyptian institutions, would not let Bernal off the hook that easily. In a discussion of the propriety and impropriety of writing, Socrates records the Egyptian tradition of an exchange between King Thamus and the god Theuth, inventor of the arts.⁶³ To Theuth's boast that he had revealed writing as a recipe for memory and wisdom, the Egyptian king rejoins with skepticism: Writing offers "no true wisdom . . . but

⁵⁸ John Peradotto, "Letter to the Editor," *Chronicle of Higher Education* (September 4, 1991): B5.

⁵⁹ Shelby Steele, "The Recoloring of Campus Life," *Harper's* (February 1989): 47–55, reprinted in Steele, *The Content of Our Character: A New Vision of Race in America* (New York, 1990).

⁶⁰ Baines, "Was Civilization Made in Africa?"; Ray, "Egyptian Perspective"; compare Lefkowitz, "Not Out of Africa." See *Black Athena*, 1: 433–37.

⁶¹ Bernal, *Black Athena*, 1: 436–37; Anderson, "Myth of Hellenism"; Giannaris, "Rocking the Cradle," 13.

⁶² Bernal, *Black Athena*, 2: xvi.

⁶³ On Plato, see Bernal, *Black Athena*, 1: 105–08, 197, 493 n. 3 *et passim*; on Theuth (or Thoth), see 1: 106, 139–45.

only its semblance, for by telling them of many things without teaching them you will make them seem to know much, while for the most part they know nothing, and as men filled not with wisdom, but with the conceit of wisdom, they will be a burden to their fellows." Furthermore, "once a thing is put in writing, the composition, whatever it may be, drifts all over the place, getting into the hands not only of those who understand it, but equally of those who have no business with it; it doesn't know how to address the right people, and not address the wrong. *And when it is ill-treated and unfairly abused it always needs its parent to come to its help, being unable to defend or help itself.*"⁶⁴

Thus, while it is accurate and morally correct to distinguish between Bernal and those extremists who misuse his arguments to promote racism, Bernal's repeated disclaimers of responsibility for the misuse of his words ignore his moral responsibility, the responsibility of the parent to come to the help of his writing "when it is ill-treated and unfairly abused."⁶⁵ To date, Bernal's response has been to state publicly (as he has in print from the start) that, despite the misleading and still unchanged title, he does not mean to imply that Egyptian civilization as a whole was black, as the term is understood today: "It was a thoroughly mixed population that got darker and more Negroid the further up the Nile you went . . . though few Egyptians could have bought a cup of coffee in America's Deep South in 1954."⁶⁶ True enough.

But I, for one, miss in Bernal's demurrals the conspicuous passion and moral fervor he brings to bear on the deplorable racism and anti-Semitism of "white," "Eurocentric" scholarship. This is especially puzzling and disappointing in one who from the start has eschewed the posture of academic objectivity or ideological neutrality. His unambiguously moralistic statement of *Black Athena's* political purpose "is, of course, to lessen European cultural arrogance." His enemy is the ideal of racial purity as opposed to pluralism. "Even if I were to concede . . . relativism [that is, the impossibility of arriving at any absolute historical truth]," he states, "I would argue that the scheme set out in *Black Athena* is better on *ethical grounds*, that it is more congenial to our general preferences—to the general liberal preferences of academia—than that of the Aryan Model."⁶⁷

The entirely admirable attempt to remove the racist distortions and excesses of nineteenth-century classical scholarship—in which Volume 1 of *Black Athena* has taken center stage—bears with it the responsibility to respond critically to revisionist versions no less ugly for their distortions and polemicism and equally frightening in their fanaticism. Two wrongs do not make a right. Two wrongs are always no more than one wrong times two. Racism kills, heedless of moral symmetries, and it is small consolation for any victim of racism to be told that his or her death is historically insignificant: "Yes, I do believe that Afrocentric

⁶⁴ Plato, *Phaedrus* 275b, 275e, R. Hackforth, trans., *The Collected Dialogues of Plato*, Edith Hamilton and Huntington Cairns, eds. (Bollingen Series 71) (Princeton, N.J., 1961), 520–21, italics mine.

⁶⁵ Bernal is quoted as unhappy with those people who have not opened the book but "who have used it as a talisman of an unthinking Afrocentrism"; Ellen K. Coughlin, "In Multiculturalism Debate, Scholarly Book on Ancient Greece Plays Controversial Part," *Chronicle of Higher Education* (July 31, 1991): A6; compare Bernal, "Letter," *New Republic* (March 9, 1992): 5.

⁶⁶ "Out of Egypt, Greece," *Newsweek*, 50. Compare Bernal, "Black Athena and the APA," 30–32; Bernal, "Response to Dr. John Ray," *Journal of Mediterranean Archaeology*, 3 (1990): 118–19.

⁶⁷ Bernal, *Black Athena*, 1: 73; Bernal, "Black Athena and the APA," 25, italics mine.

scholars can be as biased and dogmatic as Eurocentric scholars. I'm less worried by it because whites have nearly all the power at the moment, and though I don't care for black racism I don't think that it is a menace to society in the way that white racism is."⁶⁸ Whether current racist distortions are the result of good intentions or deliberately self-serving, the author of *The Fabrication of Ancient Greece* has a moral obligation to respond to all fabrications of ancient Greece, no matter what quarter they come from.

Instead, Bernal is distancing, temporizing, and subtly redirecting his goals away from morality and back to his own chimera of purely "objective" scholarship:

Some of my classicist friends have asked me whether I am not disturbed by the uses made of *Black Athena* by Black racists. My answer to this is that I am disturbed because I hate racism of any kind. I would prefer to be in my position than theirs, however, as I am infinitely less concerned by black racism than I am by white racism, and white racists, directly or indirectly, make constant use of orthodox views of the classical world and the Aryan Model. *In any event, regardless of the politics of the situation, the reason why I am devoting the second half of my life to this project is not simply as an attack on white racism but because I believe the Revised Ancient Model to be a less inaccurate representation of the history with which it is concerned and I know that untangling its ramifications is fascinating.*⁶⁹

Putting the genie back in the bottle (or "toothpaste back in the tube," Bernal on himself, quoting H. R. Haldeman) is no option.⁷⁰ For better or worse, Bernal has moved the arguments of radical Afrocentrists from the wings to center stage and must live with the consequences.⁷¹ This means that he must be as outspoken in condemning the racist excesses of some Afrocentrists as he is in condemning white racism.

Regardless of the slow reflexes of the professionals or Bernal himself, there can be little debate regarding the popular impact of *Black Athena*. The plethora of reviews, articles, citations in the popular press and even a television movie attest to the phenomenal popular impact of the book. Subject of countless symposia and debates in this country and abroad, Bernal has come a long way from his first rejections by university presses in the United States.⁷² *Black Athena* is an academic best seller—winner of the 1990 American Book Award and money maker for its publishers.

In 1987, when I first read Volume 1, I thought that it was a good story; four years later, rereading the book, I still do. Then as now, the book was that *rara avis*—an academic page-turner—in which every element of the enormously complex plot meshed perfectly to create a story with good guys (Herodotus, Egyptians, Semites) and bad guys (Aryans, racist German philologists); in which the hero-author, the indefatigable neophyte detective, rereads the files and

⁶⁸ Bernal, in Allen, "Black Athena: An Interview," 21.

⁶⁹ Bernal, *Black Athena*, 2: xxii, italics mine.

⁷⁰ Bernal, *Black Athena*, 2: xxii.

⁷¹ Thomas C. Patterson, "Another Blow to Eurocentrism," *Monthly Review: An Independent Socialist Magazine*, 40 (December 1988): 45. For arguments of radical Afrocentrists, see sources cited in n. 42. For contrasting appraisals of radical Afrocentrism, see Andrew Sullivan, "Racism 101: A Crash Course in Afro-centrism," *New Republic* (November 26, 1990): 20–21; and Tate, "History: The Colorized Version."

⁷² Bernal, *Black Athena*, 2: xvi–xvii.

reworks the clues to uncover the truth that had long been covered up by a contemptibly corrupt and lazy police department (contemporary classicists). I was completely engaged by the passion and moral fervor of the book. I also feared the way in which it might be used to buttress black racism. But as a classicist and teacher of the very groups (African Americans at Howard and Jews at Bar-Ilan) whose contributions to “the glory that was Greece” allegedly had been been slighted in the historical record, I was above all morally compelled to know whether Bernal’s story was not only riveting but true. In organizing the American Philological Association panel that first brought the book before the “classics establishment,” I looked to the experts for substantive critiques (which they, for the most part, have not provided), and above all I looked to Martin Bernal to persevere in the role of whistleblower on racist distortions in historiography, irrespective of their origins.

Today—an ocean of reviews, symposia, and speeches later—I still am not much smarter, but I am more afraid and more cynical. Does Bernal’s new paradigm for Greek prehistory synthesized from disparate and varied strands of evidence represent a truer version of “the way things were”? Or is *The Fabrication of Ancient Greece* itself the adroit fabrication of a skilled storyteller to be used for still more fabrications? And is it too late to plead for all fabrications to yield to a truer version of historical reality?

Review Article
Black Athena 2: History without Rules

ROBERT L. POUNDER

Martin Bernal, **Black Athena: The Afroasiatic Roots of Classical Civilization**, Volume 2: **The Archaeological and Documentary Evidence** (New Brunswick, N.J.: Rutgers University Press, 1991). 736 pp. \$60.00, paper \$15.95.

MARTIN BERNAL ESTABLISHES THE METHODOLOGICAL FOUNDATIONS of this book, as well as of its predecessor and the two volumes that are promised, early in his introduction: "I have based my case in this whole project on the principle of competitive plausibility rather than certainty."¹ Undergraduate term papers are failed every day for indulging in "competitive plausibility," and unless we are willing to endorse a fanciful and revolutionary approach to the writing of history, we must, in examining *Black Athena*, subject its arguments to a level of skepticism, and indeed to the application of evidential rigor, that such methodology demands.

Bernal's claim is that certainty cannot be achieved in his investigations. One could argue that certainty is elusive in any period of history, that subjective interpretation defines history. But all important historical analysis incorporates, to the extent it can, concrete, "hard" evidence, of various sorts. At the very least, hard evidence should not be ignored when it exists and is available. Nor does Bernal ignore it, as long as it suits his particular purposes. But he castigates "archaeological positivists," who tediously prefer to see some sort of physical indication before they are willing to draw conclusions, especially conclusions that sweep aside decades of painstaking physical and mental labor.

This is a long book. To illustrate the methodology sketched above, it may be instructive to refer to two chapters (2 and 3) dealing with Egyptian influence on Boeotia and the Peloponnese in the third millennium B.C. Here, Bernal argues that similarities between Egyptian mythology and Boeotian cults indicate the presence of Egyptians in Boeotia in the early Bronze Age (the third millennium B.C.). On this point, let it be said that the myths of many cultures share common themes, even individual details of stories, so we are provided a slender reed with which to overturn traditional chronology. The further claim, that myths of

¹ Martin Bernal, *Black Athena*, Volume 2: *The Archaeological and Documentary Evidence* (New Brunswick, N.J., 1991), 3–4.

Herakles as a hydraulic engineer should be meshed with achievements in irrigation and drainage in Middle Kingdom Egypt to prove the presence of Egyptian engineers in Boeotia in the third millennium, is given no substance apart from vague coincidence. The draining of the Kopaic Basin, currently dated, on the basis of ceramic and architectural evidence, to the Mycenaean (late Bronze) period, should not be pushed back a thousand years on grounds of a dubious plausibility and without a scrap of physical corroboration (“[a date in] the 3rd millennium would seem plausible despite the lack of any definitively Egyptian objects from Boiotia in that period.”²

An Egyptian presence on the Greek mainland in the third millennium B.C. is an important aspect of Bernal's assertions. The first chapter of the book, however, deals with Crete before the Palace period (7000–2100 B.C.). During these centuries, according to Bernal, who rejects theories of indigenous development (not only on Crete but in the Aegean region generally), there was massive and sustained influence from the Near East and Egypt. It is doubtful that any Minoan archaeologist today would deny the existence of some external influence in pre-palatial Crete, especially from the Levant, but there are no physical remains, at least so far, to support the notion of widespread contacts or occupation. Bernal's polarization of the “diffusionist” explanation of cultural development in the prehistoric Aegean (championed in the 1930s by V. Gordon Childe, to whom the volume is dedicated) and the “isolationist” explanation creates unnecessary barriers that are not supported by the very scholars he cites and sets in opposition (Keith Branigan, Saul Weinberg, Lucy Goodison, among others, on the one hand, and Colin Renfrew and Peter Warren, on the other). He is quite open about his distrust of most contemporary archaeologists, who, he claims, are tainted by an anti-Semitic and racist heritage that dates to late eighteenth-century Germany (a theory expounded in *Black Athena*, Volume 1), but he repeatedly skirts a serious refutation of the conclusions they draw from concrete evidence.³ Instead, he inflates the role of Egypt beyond its borders on the basis of an ill-defined plausibility.

Chapters 4 through 12 examine aspects of purported Egyptian and Levantine influence on Greece in the Bronze Age. The most striking feature of Bernal's arguments in these pages is a resurrection of theories popular in the 1920s and 1930s (espoused by Childe, for example) that made the Hyksos, a shadowy foreign group that dwelt in the Nile delta in the first half of the second

² Bernal, *Black Athena*, 2: 135.

³ There are many instances of this throughout the volume, and space does not permit a full examination of them. One example, however, is Colin Renfrew's theory (expounded in *The Emergence of Civilisation: The Cyclades and the Aegean in the Third Millennium B.C.* [London, 1973], 110) regarding Early Bronze Age buildings in Boeotia and the Argolid that he believes are private houses. Bernal, in his hope to associate the structures with hydraulic works that are conventionally dated much later, asserts that they are granaries. Another example is Bernal's tinkering with Egyptian chronology as he borrows from the work of scholars whose careers span the twentieth century, from James Breasted at its beginning, through Hans Stock and Eduard Meyer to James Mellaart and Herbert Haas in recent years. The result of his convoluted conflation of these scholars' theories and research is an affirmation of Breasted's early dates for the 7th through 11th dynasties (J. H. Breasted, ed., *Ancient Records of Egypt: Historical Documents from the Earliest Times*, vol. 1 [Chicago, 1906], 40–45). Bernal's purpose in this exercise (*Black Athena*, 2: 235) is to assign an active role in the eastern Mediterranean to Sesostrius I, “despite the lack of rigour in this eclectic procedure.”

millennium B.C., the dominant force in the eastern Mediterranean. Much more is now known about them, thanks to the Austrian excavations of the 1960s at the site of Tell ed-Daba'a, which the excavators identify with the Hyksos capital of Avaris. The excavators also report that the pottery and architecture found at this delta site point to a Canaanite origin for the Hyksos, who oversaw a thriving trade in wine and oil. According to Bernal, this purely mercantile role for these newcomers to Egypt from the Levant should be rejected. They should be seen, along with the Hurrians and (oddly) the Indo-Europeans who, he believes, commingled with them, as warlike invaders ruling a vast empire from Anatolia to Greece. In Greece, so the theory goes, they became the initiators of Mycenaean civilization. Bernal is at pains to explain how he can envision a role for Indo-European Aryans in the composition of the Hyksos, but his explanation serves only to confuse: "where I accept the Aryanists' interpretation, I refuse to accept their basic Social Darwinist premise that conquest or domination through violence somehow makes a people or linguistic group morally or creatively *better* than those who are conquered or dominated."⁴ The time is long gone when ancient historians considered violent invaders to be morally superior to those they subdued. On the contrary, to mention one example, for many decades the "peaceful" inhabitants of Minoan Crete in the Middle Bronze period have been celebrated and romanticized, perhaps excessively, by such scholars as J. Walter Graham, following the lead of the archaeologist of Knossos, Sir Arthur Evans.⁵

Just as troubling as the importance Bernal assigns to the Hyksos is his overarching belief in forays by pharaonic Egypt into the Near East and Greece throughout the Bronze Age. These phantom armies were comprised mainly of blacks, says Bernal: they were African armies. Reviewers of the first volume have dealt at length with the difficulties in finding reasons to support a significant role for blacks in Egyptian society.⁶ For instance, there is nothing wrong with the notion of a black Cleopatra, except that we have no reason to believe she was anything other than a Ptolemaic Greek with a Macedonian lineage. That is what the evidence conveys. Bernal makes a major contribution to confusion and divisiveness by giving credence to Afrocentrist theories that cannot be supported by historical, anthropological, or archaeological criteria.

Thus the heart of the flaw in Bernal's ambitious project. In his ardor to convince us that Egypt and the Near East played the essential role in the development of Greek culture, not just through trade and occasional immigration but through colonization and conquest, he sets aside standards of evidence that are not the twisted constructs of evil German historians of the nineteenth century; rather, they find their origins at least as early as Thucydides. These standards are

⁴ Bernal, *Black Athena*, 2: 360.

⁵ J. Walter Graham, *The Palaces of Crete*, 2d edn. (Princeton, N.J., 1972).

⁶ The first volume was widely reviewed and discussed, for instance, by Martha A. Malamud, *Criticism*, 31 (1989): 317–22; Jasper Griffin, *New York Review of Books*, June 15, 1989; in a special review section by James D. Muhly, Sarah Morris, Patricia Maynor Bikai, J. D. Ray (and with a response by Martin Bernal), in *Journal of Mediterranean Archaeology*, 3 (June 1990): 52–110; and in a special issue, John Peradotto, *et al.*, "The Challenge of 'Black Athena,'" *Arethusa* (Fall 1989). There have also been many articles in the popular press.

true ancient models. Their dictate is simple: base your assertions on evidence, not on bias or wishful thinking.

BERNAL'S WORK AND THE STIR IT HAS OCCASIONED have caused ancient historians and archaeologists to undertake a major reexamination of methods and motives. His charges of anti-Semitic and racial prejudice among nineteenth-century scholars can hardly be refuted. One can find numerous patronizing, paternalistic, or outright racist references to blacks, Jews, and Arabs, not to mention sexist treatment of women, in the journals. We would be surprised if these slurs were not there, as the prejudice they reflect stems from attitudes that were, and to a degree still are, part of the fabric of European and North American society. The reason one cannot, in the end, accept his theories is because they force the conclusion that racist historians counterfeited evidence. However imperfectly they may have construed evidence, however selectively they emphasized it, the evidence itself remains. And it does not permit the interpretation Bernal wants. Thus, in reexamining the past, we must not cast aside the real for the likely, the known for the hoped-for. The enormous learning that has produced this book founders on methodological weakness and stubborn idealism.

Bernal puts himself in the company of Heinrich Schliemann and Michael Ventris, outsiders who changed the landscape of ancient studies.⁷ But Schliemann found Mycenae and Troy, and Ventris discovered that Linear B was a syllabic script for an early form of Greek. These are tangible accomplishments based on tangible evidence. Bernal is more like the armchair philosopher whose views are imbued with a plausibility that is far too private, whose stated motives seem at odds with the strange fruits of his labors.

⁷ Schliemann made a fortune in trade before turning to archaeology; Ventris was trained as an architect.

Review Article
Slavery in Africa and the Slave Trades from Africa

JANET J. EWALD

William Gervase Clarence-Smith, ed., **The Economics of the Indian Ocean Slave Trade in the Nineteenth Century** (London: Frank Cass, 1989). 222 pp.

Paul E. Lovejoy, **Transformations in Slavery: A History of Slavery in Africa** (Cambridge: Cambridge University Press, 1983). 349 pp.

Patrick Manning, **Slavery and African Life: Occidental, Oriental and African Slave Trades** (Cambridge: Cambridge University Press, 1990). 236 pp.

Suzanne Miers and Richard Roberts, eds., **The End of Slavery in Africa** (Madison: University of Wisconsin Press, 1988). 524 pp.

Joseph C. Miller, **Way of Death: Merchant Capitalism and the Angolan Slave Trade, 1730–1830** (Madison: University of Wisconsin Press, 1988). 770 pp.

Richard L. Roberts, **Warriors, Merchants, and Slaves: The State and the Economy in the Middle Niger Valley, 1700–1914** (Stanford: Stanford University Press, 1987). 293 pp.

Abdul Sheriff, **Slaves, Spices and Ivory in Zanzibar** (Athens: Ohio University Press, 1987). 297 pp.

THE ATLANTIC SLAVE TRADE was not Africa's only slave trade. Enslaved Africans also crossed the Sahara Desert, Red Sea, and Indian Ocean.¹ Most of the slaves who traveled these routes ended their journeys in Muslim societies. Africa's slave trade to the Islamic world began centuries before the Atlantic slave trade and lasted somewhat longer, in some places into the twentieth century. During the

I would like to acknowledge the useful comments of Judith Bennett, Steven Feierman, John Hanson, and Cynthia Herrup.

¹ In this essay, by "Atlantic Africa" I mean those parts of sub-Saharan Africa that sustained an intercontinental exchange of goods or slaves via the Atlantic. By "Islamic Africa" I mean those parts of the continent south of the Sahara where Muslims exercised significant cultural, economic, or political influence; Islamic Africa, therefore, was not a homogeneous bloc. I recognize that these two categories are not equivalent; that is one reason why, as I argue below, it is difficult to compare slavery and the slave trade in Atlantic and Islamic Africa. In addition, I recognize that no clear or unchanging boundary separated "Islamic" from "non-Islamic" Africa, or the Atlantic slave trade from the slave trade to the Islamic world. For example, the Muslim merchants described by Richard Roberts exported goods both north, across the Sahara to the Islamic world, and south and west, toward the Atlantic. And slaves passed through the hands of Muslim traders on their way to French plantations in the Indian Ocean.

thousand years this trade lasted, perhaps as many (or even more) slaves crossed the Sahara and eastern oceans as crossed the Atlantic in under four hundred years.² As in the Atlantic trade, the same networks taking millions of slaves out of Africa also transported others within the continent. Many of these enslaved men and women found themselves in African Muslim communities.

Despite its scope and longevity, slavery in Islamic Africa, and Africa's slave trade to the external Islamic world, has not engaged European and American scholars as intensely as has slavery in the Atlantic world.³ Historians recognize Atlantic slavery as binding the histories of three continents and as posing questions for our communities, both past and contemporary. As a scholarly community, historians have inherited the pressing questions about slavery asked by Europeans, Africans, and Americans over the centuries. During the age of the French and American revolutions, some Europeans and Euro-Americans increasingly queried whether slavery belonged in what they saw as progressive, and perhaps even perfectible, Western societies.⁴ The African slaves challenged slavery by rebelling—most successfully in Saint Domingue. Inheriting and reworking the message of both abolitionists and slaves, Afro-Caribbean thinkers of the mid-twentieth century tested the meaning of American slavery for the rise of European democracy and capitalism.⁵ Debates about slavery then traveled back to Atlantic Africa from the Caribbean. A Guyanese historian, Walter Rodney, argued that the Atlantic slave trade distorted economies and stimulated slavery within Africa. Philip Curtin, who began his career with a book about Jamaica, compiled the first reliable census of the Africans who made the Middle Passage. Rodney and Curtin sparked debates that often became vehement. But the debates were also coherent, focusing on clear questions: how the Atlantic slave trade

² Quantifying Africa's slave exports across the Sahara, Red Sea, and Indian Oceans is much more difficult than quantifying the Atlantic slave trade. Paul Lovejoy estimates a total of 11,612,000 slaves crossing the Red Sea, Indian Ocean, and Sahara Desert from 650 to 1900; from 1500 to 1900, he estimates that 11,656,000 slaves crossed the Atlantic. Paul Lovejoy, *Transformations in Slavery: A History of Slavery in Africa* (Cambridge, 1983), 25, 44, 137. Ralph A. Austen suggested that 17,000,000 African slaves crossed the Sahara and eastern oceans. Austen, *African Economic History: Internal Development and External Dependency* (Portsmouth, N.H., 1987), 275. For Austen's recent findings on the nineteenth-century Red Sea and Indian Ocean slave exports, as well as a short analysis of the difficulties and significance of such quantitative work, see his essay, "The 19th Century Islamic Slave Trade from East Africa (Swahili and Red Sea Coasts): A Tentative Census," in W. G. Clarence-Smith, ed., *The Economics of the Indian Ocean Slave Trade in the Nineteenth Century* (London, 1989), 21–44.

³ Scholars representing a range of perspectives agree on this point. John Ralph Willis, "Preface," in Willis, ed., *Slaves and Slavery in Muslim Africa. Volume 1, Islam and the Ideology of Enslavement* (London, 1985), vii; Patrick Manning, *Slavery and African Life: Occidental, Oriental and African Slave Trades* (Cambridge, 1990), 13; W. G. Clarence-Smith, "The Economics of the Indian Ocean and Red Sea Slave Trades in the 19th Century: An Overview," in Clarence-Smith, *Economics of the Indian Ocean Slave Trade*, 1.

The contents of the journal *Slavery and Abolition* reflect the position of scholarship about slavery and the slave trade in the Islamic world, including Islamic Africa. Of the fifteen contributions to the three issues of 1990, none treated the African slave trade to the external Islamic world. One dealt with slavery in the setting of an African Islamic society. The journal itself, however, has encouraged scholarship about Islamic Africa. The volume here under review edited by Clarence-Smith first appeared as a special issue of *Slavery and Abolition*, 9, no. 3 (1988).

⁴ The authoritative account of the origins of this debate is David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975).

⁵ C. L. R. James, *Toussaint L'Ouverture and the San Domingo Revolution*, 2d edn. (New York, 1963); Eric Williams, *Capitalism and Slavery* (Chapel Hill, N.C., 1944).

affected Africa, how Africans exploited slaves, and what numbers of Africans crossed the Atlantic as slaves.⁶

No similar intercontinental perspective or central questions dominate our inquiries into slavery and the slave trade in the presence of Islam. Several reasons account for this. A more diffuse scholarship has resulted in part from a more diffuse slave trade. Unlike the Atlantic trade, which sustained a coherent regional system of exchange from the sixteenth century, no single regional network dominated Africa's slave exports to the Islamic world. Instead, multiple and overlapping networks—rising, declining, and sometimes flourishing again over a millennium—tied various areas of Africa to various areas of the external Islamic world. Partly as a result of the antiquity and complexity of this trade, we find it much more difficult to count the numbers of slaves exported to the Islamic world than to the Americas. In addition, Americans and Europeans have not inherited scholarly questions about Africa's slave trade to the Islamic world in part because most of us do not feel ourselves the heirs of an Islamic tradition. Moreover, slavery did not preoccupy Muslim intellectuals in the same way that it worried eighteenth and nineteenth-century European and North American thinkers.⁷ Although slaves of Muslims resisted, as did slaves everywhere, nowhere in the modern Islamic world did slaves mount a revolution like that in Saint Domingue. Nor did the twentieth-century Islamic world produce an equivalent of the Afro-Caribbean scholars who pondered the links among the three continents of their heritage. The historiography of slavery in Islamic Africa and Africa's slave trade to the Islamic world thus differs substantially from the historiography of Atlantic slavery and the Atlantic slave trade.

The issue of comparative history then arises: how can we put all of Africa's slave

⁶ What follows is, in chronological order, a sample of this debate as it has been conducted in the *Journal of African History* (hereafter, *J Afr H*), as well as a few seminal books: Walter Rodney, "African Slavery and Other Forms of Social Oppression on the Upper Guinea Coast in the Context of the Atlantic Slave Trade," *J Afr H*, 7 (1966): 431–43; John D. Fage, "Slavery and the Slave Trade in the Context of West African History," *J Afr H*, 10 (1969): 393–404; Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wis., 1969); J. E. Inikori, "Measuring the Atlantic Slave Trade: An Assessment of Curtin and Anstey," *J Afr H*, 17 (1976): 197–233; Philip Curtin, Roger Anstey, and J. E. Inikori, "Discussion: Measuring the Atlantic Slave Trade," *J Afr H*, 17 (1976): 595–627; John D. Fage, "Slaves and Society in Western Africa, c. 1445–c. 1700," *J Afr H*, 21 (1980): 289–310; J. E. Inikori, ed., *Forced Migration: The Impact of the Export Slave Trade on African Societies* (New York, 1982). Inikori's collection brings together relevant essays, most of them published previously elsewhere; Paul E. Lovejoy, "The Volume of the Atlantic Slave Trade: A Synthesis," *J Afr H*, 19 (1982): 473–502; David Richardson, "Slave Exports from West and West-Central Africa, 1700–1810: New Estimates of Volume and Distribution," *J Afr H*, 30 (1989): 1–22; David Geggus, "Sex Ratios, Age and Ethnicity in the Atlantic Slave Trade: Data from French Shipping and Plantation Records," *J Afr H*, 30 (1989): 23–44; Robin Law, "Slave-Raiders and Middlemen, Monopolists and Free-Traders: The Supply of Slaves for the Atlantic Trade in Dahomey, c. 1715–1850," *J Afr H*, 30 (1989): 45–68; Paul E. Lovejoy, "The Impact of the Atlantic Slave Trade on Africa: A Review of the Literature," *J Afr H*, 30 (1989): 365–94; David Geggus, "Sex Ratio and Ethnicity: A Reply to Paul E. Lovejoy," *J Afr H*, 30 (1989): 395–98; David Eltis, "The Volume, Age/Sex Ratios, and African Impact of the Slave Trade: Some Refinements of Paul Lovejoy's Review of the Literature," *J Afr H*, 31 (1990): 485–92.

⁷ Bernard Lewis argues that Muslim scholars could not take an abolitionist stance, for to do so would be to "forbid what God permits"; Lewis, *Race and Slavery in the Middle East: An Historical Inquiry* (New York, 1990), 78. The legality of enslavement, however, did become a subject of intense debate among Muslims. This debate was cited by the bey of Tunis, the first Muslim ruler to abolish slavery; Lewis, 79. For Africa specifically, the Islamic ideology of enslavement is a central theme in Willis, *Slaves and Slavery in Muslim Africa*, vol. 1.

trades, as well as slavery in Islamic and non-Islamic Africa in a common framework?⁸ In exploring this issue, I do not intend to survey the scholarship about Islam, the slave trade, and slavery in Africa. Rather, I limit my discussion to seven recently published books about slavery in Africa and slave trades from Africa. The range of approaches to Africa's slave trade to the external Islamic world and slavery in Islamic Africa is not represented in these books.⁹ Instead, these works belong to a specific scholarly tradition. Grounded in the social sciences and inclined to comparisons, this scholarship calls on the different but complementary research of Philip Curtin and the French anthropologist Claude Meillassoux.¹⁰ Both Curtin and Meillassoux worked in that part of West Africa where Islamic and Atlantic Africa overlapped. Like both Meillassoux and Curtin, several of the authors under review address the economies of both Islamic and non-Islamic communities. Like Curtin, almost all of the scholars specialize in the social and economic history of Africa rather than in studies of Islam.¹¹

The books under review thus test the possibilities of building a comparative framework for slavery and the slave trade, in Atlantic as well as Islamic Africa. Finally, when they address the Atlantic slave trade and slavery in Atlantic Africa, the authors engage in a dialogue addressing and advancing a set of established, shared issues. Extending common questions to Islamic Africa and the slave trade to the external Islamic world, however, remains more difficult. The books under review nonetheless suggest comparative frameworks for all of Africa's slave trades and slavery in Atlantic as well as Islamic Africa. Finally, when writing about both Atlantic and Islamic Africa, several of the authors reveal that struggles over the end of slavery created new ideologies and identities.

⁸ Frederick Cooper pointed out the value of comparative studies when, more than a decade ago, he called on historians of Africa to pay more attention to scholarship about slavery in the Americas. Cooper, "The Problem of Slavery in African Studies," *JAfrH*, 20 (1979): 103, 105, and *passim*.

⁹ In particular, this essay does not examine the works of scholars who analyze slavery and the slave trade from the perspective of their specialization in Islamic societies. For outstanding examples of this approach, see Allan G. B. Fisher and Humphrey J. Fisher, *Slavery and Muslim Society in Africa: The Institution in Saharan and Sudanic Africa and the Trans-Saharan Trade* (Garden City, N.Y., 1971); Willis, *Slaves and Slavery in Muslim Africa*, vol. 1, and vol. 2: *The Servile Estate* (London, 1985).

¹⁰ For Curtin, see especially *Atlantic Slave Trade*; Philip D. Curtin, *Economic Change in Precolonial Africa*, 2 vols. (Madison, Wis., 1975); Philip D. Curtin, *The Rise and Fall of the Plantation Complex: Essays in Atlantic History* (Cambridge, 1990). For Meillassoux, see especially his edited volume, *L'esclavage en Afrique précoloniale* (Paris, 1975); Claude Meillassoux, "The Role of Slavery in the Economic and Social History of Sahelo-Sudanic Africa," in Inikori, *Forced Migration*, 74–99; "Correspondence," in *Economy and Society*, 7 (1978): 312–31; "Female Slavery," in Claire C. Robertson and Martin A. Klein, eds., *Women and Slavery in Africa* (Madison, Wis., 1983), 49–66; *L'anthropologie de l'esclavage: Le ventre de fer et d'argent* (Paris, 1986); a translation of which has been issued recently as *The Anthropology of Slavery: The Womb of Iron and Gold* (Chicago, 1991).

¹¹ Particularly apparent are the ties, through the University of Wisconsin, of Philip D. Curtin to many of the authors of the books under review. Manning, Miller, and Lovejoy received doctoral degrees from Wisconsin—and Miers held a temporary appointment there—during Curtin's time at that university. During roughly the same time, Abdul Sheriff spent a year at Wisconsin, where he began the dissertation revisions that resulted in his book. This shared background has resulted in a common ground for debate rather than agreement on every issue. In this essay's section on Atlantic Africa, I delineate how Manning, Miller, and Lovejoy differ in their approaches. In their reactions to Claude Meillassoux's *L'anthropologie de l'esclavage*, Miller and Lovejoy crystallize some of the differences in their own approaches: Joseph C. Miller, "The World according to Meillassoux: A Challenging but Limited Vision," *International Journal of African Historical Studies*, 22 (1989): 473–95; Paul Lovejoy, "Miller's Vision of Meillassoux," *International Journal of African Historical Studies*, 24 (1991): 133–45.

Central to the essay are two syntheses: *Transformations in Slavery* by Paul Lovejoy and *Slavery and African Life* by Patrick Manning. These two books reflect the changing scholarship during the 1980s about slavery and the slave trade. Each claims to treat all of the continent below the Sahara, including both Atlantic and Islamic Africa. Case studies from Atlantic and Islamic Africa appear in two collected volumes. *The End of Slavery in Africa*, edited by Suzanne Miers and Richard Roberts, analyzes not only emancipation but also various forms of pre-colonial slavery among Africans. *The Economics of the Indian Ocean Slave Trade in the Nineteenth Century*, edited by W. G. Clarence-Smith, examines how African slaves moved toward and across the Indian Ocean—a domain including the European-ruled Indian Ocean islands and Cape of Good Hope, which were extensions of the Atlantic world.¹² Two case studies examine Islamic Africa and the slave trade to the Islamic world. The Indian Ocean trade, centered on the African island of Zanzibar, forms the subject of *Slaves, Spices and Ivory in Zanzibar* by Abdul Sheriff. Richard Roberts, in *Warriors, Merchants, and Slaves*, examines three successive states in the middle Niger—one of them an Islamic theocracy. Finally, Joseph C. Miller's *Way of Death* traces the flows of slaves and merchant capital across the Atlantic among Angola, Brazil, and Portugal.

A ubiquitous term is "transformation," although the various authors often part company on what is being transformed and how. For Atlantic Africa, however, scholars debate about two great transformations: how did the slave trade and slavery in the Americas nurture Europe's transformation to industrial capitalism?¹³ And how did the Atlantic slave trade transform African societies? These two questions, derived from the Atlantic world and central to Miller's book, also inform in different degrees the syntheses by Lovejoy and Manning. Claiming to treat all of sub-Saharan Africa, Lovejoy and Manning nonetheless add most to the already rich scholarly heritage of the Atlantic world.

ENSLAVED AFRICAN MEN AND WOMEN crossing the Atlantic linked the transformations of two continents. For Europe, some believe that African slaves—whether as workers or commodities to be bought and sold—aided the transformation to industrial capitalism. More certainly, African societies underwent an equally profound, though radically different, set of changes. At roughly the same time that wage labor came to dominate Europe, slavery became entrenched within much of Africa. Slavery in Atlantic Africa reached a peak after the abolition of the ocean trade, when slaves produced much of the grain, indigo, cloth, peanuts, palm oil, and other commodities essential for consumption and trade.

Lovejoy's 1983 synthesis is a landmark, for it reveals both the economic importance of slaves and the depth of the transformation within Africa.¹⁴ Titling

¹² The essays in *Economics of the Indian Ocean Slave Trade* were first presented at a workshop held at the School of Oriental and African Studies in 1987, University of London. I presented a paper there, a revised version of which appeared in both the special issue of *Slavery and Abolition*, 9, no. 3 (1988) and in *Economics of the Indian Ocean Slave Trade*.

¹³ By no means all scholars agree that slavery helped Europe make the transformation to industrial capitalism. See Curtin, *Rise and Fall of the Plantation Complex*, 204–05.

¹⁴ For an early work discussing the importance of slaves and the transformation of production in

his book *Transformations in Slavery*, Lovejoy cites M. I. Finley, who wrote that "slavery . . . is transformed . . . when slaves play an essential role in the economy." Such a transformation, argues Lovejoy, occurred in Africa as slavery went from being a marginal feature of society to being vital to production. Drawing on French neo-Marxist scholarship, Lovejoy uses the concept of mode of production: a lineage or domestic mode was transformed into a slave mode.¹⁵ The transformation, according to Lovejoy, emerged from the interaction between internal dynamics and external forces. Lovejoy does not, however, suggest that African slaves or the slave trade transformed the rest of the world but instead simply notes that "Africa responded to outside influences [more] . . . than it influenced the outside world."¹⁶

In his synthesis, which appeared seven years after Lovejoy's book, Patrick Manning considers transformations in both Europe and Africa. For Africa, Manning generally agrees with Lovejoy's argument that slaves became essential to African economies. But he adds new quantitative evidence, the most important since Curtin, about the impact of the slave trade. Using first a static demographic model, then a computer simulation to reveal change over time, Manning painstakingly delineates how the external slave trades affected populations within the various regions of Africa. For the entire continent, Manning concludes that if slaves had not been forced to emigrate, Africans would have numbered almost 100 million instead of 50 million in 1850.¹⁷

Unlike Lovejoy, Manning asks how African slaves in the Americas nurtured the capitalist transformation. And why, he further queries, did Africa, rather than another part of the world, give up slaves for the Atlantic trade? When answering both of these questions, Manning stresses the value of African labor, whether in the Americas or Africa. Invoking but revising Eric Williams, Manning asserts that Africans working on plantations fueled the economic growth of the Americas and thus the transformation of the Atlantic basin. Africa, instead of another region, yielded slaves to the Americas because of differences in the productivity of labor between the two continents. In Africa, the technology of hoe agriculture depressed the value of human labor relative to the Americas. If workers in Africa had been able to produce as much as workers in other parts of the world, the prices of slaves would have become too expensive for European buyers. Low

one part of Africa, see Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, Conn., 1971).

¹⁵ Lovejoy, *Transformations in Slavery*, 9–15. When Lovejoy wrote his synthesis, historians of Africa were exploring the value of the concept of mode of production; see, for example, Donald Crummey and C. C. Stewart, eds., *Modes of Production in Africa* (Beverly Hills, Calif., 1981). A special volume of *Canadian Journal of African Studies*—19, no. 1 (1985)—devoted itself to examining the concept of mode of production. Specifically, Lovejoy bases his use of the concept on Emmanuel Terray, "Long-distance Exchange and the Formation of the State: The Case of the Abron Kingdom of Gyaman," *Economy and Society*, 3 (1974): 315–45. For a criticism of Terray's argument that a "slave mode of production" dominated other modes of production in Gyaman, see Cooper, "Problem of Slavery in African Studies," 116. See also Orlando Patterson, "On Slavery and Slave Formations," *New Left Review*, 117 (1979): 31, 47–67. Central to Meillassoux's *L'anthropologie de l'esclavage* is a discussion of how a slave mode of production was reproduced by the capture and sale of slaves; pp. 73, 90, 212, 313–14. See also Lovejoy, "Miller's Vision of Meillassoux," 134–35.

¹⁶ Lovejoy, *Transformations in Slavery*, xii.

¹⁷ Manning, *Slavery and African Life*, 85.

prices meant that slaves in the Americas quickly paid for themselves; the value of their production soon exceeded their cost.

Joseph Miller elegantly and persuasively links the two transformations, tracing the flows of capital among Angola, Brazil, and Portugal. Miller, too, asks how Atlantic slavery contributed to industrial capitalism and why Africa in particular supplied slaves. But he answers these questions not by citing slave production. Instead, the slave trade allowed Europeans to conserve and accumulate capital, which they ultimately invested in industrial technology and wages. The capitalist transformation was thus nurtured by the transformation of slaves into gold and silver. Sometimes, the conversion was direct. Between the decline of mining and the revival of agriculture, for example, Brazilians exported slaves to the Spanish in exchange for silver. But the least risky—and thus consistently most profitable—conversions were also the most indirect. The men who made the greatest gains were the Lisbon-based merchants who extended credit to both Africa and Brazil. Selling goods on credit in Africa, these merchants claimed payment in Brazil in the form of precious metals or the white gold of Brazilian sugar.

Why did trade with Africa in particular allow Europeans to accumulate capital? Not because of differences in labor productivity, according to Miller, but because different political economies dominated Africa and the rest of the world. In Europe and Asia, political economies were based on exchange values; in Africa, they were based on use values. In exchange-value economies, people sought specie (gold and silver); in use-value economies, people sought rights to the loyalty and labor of other people. Africans tried to accumulate such obligations by borrowing trade goods, especially cloth, from Europeans and using those goods to attract followers. Europeans could thus trade with Africans with minimal expenditures of gold and silver. In contrast, trade with Asia absorbed bullion, removing precious specie from the Americas and Europe.

Describing a “great transformation” in West-Central Africa, Miller alludes to Karl Polanyi, who analyzed both the rise of European capitalism and the nature of African economies. Even before slaves in Africa produced commodities for export—the “transformation” explored by Manning and Lovejoy—political economies in West-Central Africa underwent a transformation. Kings and village headmen, bound to their followers by complicated networks of mutual obligation and exchange, fell from power. In their place rose strong, armed men who gathered around them dependents and slaves—often disproportionately women. Enslaved men often continued along the routes carrying them to new owners and, sometimes, to the ships taking them away from Africa. This forced intercontinental emigration receives a very different evaluation in *Way of Death* than in *Slavery and African Life*. Where Manning sees a demographic transformation, in which slave exports reduced the regional population of Angola, Miller argues that high rates of reproduction offset the losses of Africans taken away on slave ships.

As for European merchant capital, it transformed African political economies because, Miller argues, African use-value economies were particularly vulnerable to merchant capitalism’s network of credit and debt. Africans eagerly accepted European goods on credit, parlaying cloth into the ultimate value: rights over

people. But the bargain eventually rebounded against African debtors. The entire trade suffered from a shortage of capital. When Europeans or Euro-Africans faced a liquidity crisis—which was frequently—they demanded collection on their investments in Africa. Calling in their debts, traders triggered a chain reaction of liquidation that reverberated down the links of debt and obligation in Africa. In order to pay, Africans surrendered what they most valued: dependent men, women, and children. In asserting that slaves were produced by debt and foreclosure more than by open and massive violence, Miller differs from Manning and Lovejoy. Those two authors stress how armed men captured people; thus, in the syntheses, enslavement is associated with the image of a warrior or raider. For Miller, the pivotal figure was a trader, chief, or king trying to stave off his creditors by sending people toward the Atlantic. Miller's "way of death" was a way of debt.

In Miller's hands, the pervasive theme of death implies a moral evaluation of slavery. Likewise, Manning marries quantitative analysis with moral sensibility. In his prologue, Manning establishes the themes of sacrifice and tragedy in the history of slavery; in his final chapter, he asks us to consider how we should respond to past injustice. The willingness of both authors to grapple with moral issues departs from Philip Curtin's early injunction. More than twenty years ago, Curtin acknowledged the evil of slavery but warned us not to let considerations of morality obscure our objective scholarship.¹⁸ Since then, partly through the efforts of Curtin himself, we have learned more about the Atlantic world. We have become more aware of the intellectual and moral responses of Africans themselves to Atlantic slavery and the slave trade.

Writing about the two transformations, Miller and Manning share with Africans the use of metaphors that connote venality or corruption. In his argument about the slave trade and capitalism, Miller writes that "merchant capitalism's ability to convert people into cold metal . . . constituted the Midas touch that rendered southern Atlantic slaving the Africans' way of death." Miller's metaphor uncannily echoes a story still told on the west coast of Africa. As related by Manning, Africans believed that when a slave was thrown into the sea and drowned, his corpse grew cowrie shells—a regional currency in West Africa—which were then gleaned.¹⁹ Africans, too, linked the slave trade with death and the transformation into currency. Pondering how the slave trade transformed African communities, both Manning and the Africans who escaped slavery and sometimes profited from it use the metaphor of disease. A cycle of enslavement, in Manning's words, afflicted African societies like a "communicable disease which, once introduced, tends to spread relentlessly." In Africa, where health and disease denote social well-being and malady, people conceptualized the slave trade as an illness. Miller describes how African slave traders in parts of the Kongo belonged to a special therapeutic cult that healed the sufferings caused by their commercial activities. Participating in the slave trade thus "amounted to a

¹⁸ Philip Curtin, "The Atlantic Slave Trade, 1600–1800," in F. F. A. Ajayi and Michael Crowder, eds., *History of West Africa* (New York, 1972), 1: 240–41.

¹⁹ Manning, *Slavery and African Life*, 99.

social disease.”²⁰ The Atlantic slave trade reached into the bodies of African societies and African peoples, transforming and sometimes destroying them.

SLAVERY IN ISLAMIC AFRICA and the slave trade between Africa and Islamic communities abroad present a potentially rich topic for historians seeking to compare systems of slavery and slave trades. On the one hand, we have a clear basis for comparison. Slavery and the slave trade of Islamic Africa belonged to global slave trades and systems of slavery. On the other hand, slavery in Islamic Africa and the slave trade to the external Islamic world differ from their Atlantic counterparts in ways that might illuminate our general knowledge of the subject. The Islamic world did not experience a great transformation to industrial capitalism. The plantation did not play a key role in slavery in the Islamic world outside Africa; slaves sometimes worked on plantations, but they also labored as household servants and in a wide range of other activities. Shared legal systems and religious beliefs tied Islamic Africa to the external Islamic world, a tie that Atlantic Africa did not share with the rest of the Atlantic world. We thus have a twofold task. We need to analyze the impact of Islamic law and ideology on slavery and the slave trade, but we also need to put slavery in Islamic Africa and the slave trade to the external Islamic world in a common framework with other African slaveries and slave trades. We cannot simply devote ourselves to discerning the particularly “Islamic” features of slavery, nor can we ignore the presence of Islam. Instead, we need to analyze how dynamics common to slavery and slave trades manifested themselves in both Islamic and Atlantic Africa.

This task remains unfinished in the two syntheses under review, in spite of their many other strengths and the intentions of their authors. Lovejoy and Manning clearly incorporate all of sub-Saharan Africa into their analyses. Moreover, both authors know better than to treat Islam—a body of law and religious tenets, and thus subject to contention and revision—as a timeless and universal force explaining every aspect of slavery and commerce in slaves. Manning astutely rejects the term “Islamic slave trade,” noting that such a thing no more existed than did a Christian slave trade. Lovejoy recognizes that people use ideologies in specific historical contexts when he writes that Islam served as an ideology, available to African Muslims as they reinterpreted the meaning of slavery over centuries.²¹

But the way Manning and Lovejoy treat slavery in Islamic Africa belies their own insights. In both books, the dynamics of Islam dominate the dynamics of slavery. Islamic Africa stands apart from the rest of the continent; within Islamic Africa, distinctive practices that are attributable to religion shaped slavery and the slave trade. In *Transformations of Slavery*, for example, Lovejoy draws a “sharp distinction between the slavery of kin-based societies and the slavery of Islamic law and tradition.” The presence of Islam thus produced patterns of slavery different from those generated by kin-based societies or initiated by European demand for

²⁰ Manning, *Slavery and African Life*, 107; Miller, *Way of Death*, 202.

²¹ Manning, *Slavery and African Life*, 10; Lovejoy, *Transformations in Slavery*, 220.

slaves. Manning also presents Islam itself as stimulating a particular dynamic of slavery in parts of Africa. Citing Islamic institutions and the commercial systems of the Islamic world, Manning writes that "African Islam can be hypothesized to have elicited the expansion of slavery" and that slavery expanded in two savanna states "by the same logic that sustained the institution elsewhere in the Islamic world." Thus distinctive systems of logic separated the slavery of Atlantic and Islamic Africa. In Atlantic Africa, the logic was economic—the demand of Europeans for slaves; in Islamic Africa, the logic was mainly ideological and institutional—the presence of Islam. This brings Manning and Lovejoy perilously, though unintentionally, close to reinforcing two stereotypes: first, that slavery was implicit in Islam and second, that the West (or at least its influence) is materialistic while the East (or its influence) is ideological.²²

Why do these two scholars almost reify Islam as a force shaping slavery? Not because of their perspective on Islam itself, which—their own remarks reveal—they approach with nuance and care. Instead, their treatment of Islamic Africa derives from the forms of their books: syntheses including the entire sub-Saharan continent.²³ We still know most and ask our most coherent questions about Atlantic Africa. When we scan all of Africa, the Atlantic thus tends to hold our gaze. In the two syntheses, a focus on the Atlantic world tends to blur the vision of Islamic Africa and the slave trade to the external Islamic world. Focusing on the Atlantic encourages scholars to distinguish between what is particularly African and what is not. Such a distinction carries some validity for Atlantic Africa. There, contact with Europe came so recently and with such sudden, dramatic impact that we can distinguish dynamics alien to Atlantic Africa from dynamics indigenous to that part of the continent. But an analysis of Islamic Africa suffers when we apply the external-internal distinction too broadly. Writing of the entire continent, Lovejoy argues that slavery was transformed partly by "internal forces" and partly by "external influences." One "external influence" was the European demand for slaves; before that demand, "the Islamic world" was the major external influence on African political economies. The Islamic world easily becomes "the influence of Islam."²⁴ Thus an Eastern ideology appears as having helped shape slavery in Islamic Africa, while a Western economic demand helped shape slavery in Atlantic Africa.

Manning avoids part of this problem, rightly noting that Islam was not external to much of Africa; Islamic Africa belonged to the Islamic world. But Manning

²² Lovejoy, *Transformations in Slavery*, 18; Manning, *Slavery and African Life*, 51, 29. For insights about how notions of a "supposedly materialist West" and "supposedly spiritual East" apply to our approach to slavery in Islamic Africa, as well as an examination of the ideology of slavery in one part of Islamic Africa, see Frederick Cooper, "Islam and Cultural Hegemony: The Ideology of Slaveowners on the East African Coast," in Paul E. Lovejoy, ed., *The Ideology of Slavery in Africa* (Beverly Hills, Calif., 1981), 273–74.

²³ A look at Lovejoy's other work supports the argument that a synthesis attempting to treat the entire continent impedes analysis of Islamic Africa. In his publications about one part of Islamic Africa—the Sokoto Caliphate, which became colonial northern Nigeria—Lovejoy offers fascinating insights into slavery among African Muslims. For a skillful treatment of law, ideology, and slavery, see his article, co-authored with J. S. Hogendorn, "The Reform of Slavery in Early Colonial Northern Nigeria," in Suzanne Miers and Richard Roberts, eds., *The End of Slavery in Africa* (Madison, Wis., 1988), 391–414, which I discuss at the end of this essay.

²⁴ Lovejoy, *Transformations in Slavery*, xii, 15, 17, 20, 23.

asks the same questions of both Islamic and Atlantic Africa. When he comes up with different answers, he assigns those differences to the presence of Islam. Exploring the relationship between external trade and internal exploitation of slaves, Manning convincingly argues that the Atlantic slave trade resulted in an increase in slavery in Atlantic Africa. Looking at the slave trade to the external Islamic world, Manning finds that the external demand failed to explain the exploitation of slaves within the Islamic savanna and Horn. This observation leads Manning to single out "African Islam" as impelling the expansion of slavery.

Distinguishing between the internal and external, what is African and what is not, limits our understanding of slavery, not only in Islamic Africa but in all parts of Africa. The weaknesses of the "African/not African" dichotomy emerge in the final essay of *The End of Slavery*. In "The Cultural Context of African Abolition," Igor Kopytoff wisely reminds us that many African slaves and ex-slaves experienced a subtle range of dependencies rather than a clear movement from slavery to freedom. But, on this basis, he then attempts to describe an African slavery in which kinship metaphors and relations characterized the "cultural meanings" of slavery. A "cultural universe" of shared ideas bound masters and slaves in Africa. By this criterion, according to Kopytoff, the slavery of Africans under Muslim Arab masters in Zanzibar was "slavery in Africa" but cannot be analyzed as an African system of slavery. In other parts of Africa, slavery was "duocultural," with the "interpenetration . . . of indigenous African and imported Islamic notions of slavery."²⁵

The juxtaposition of "indigenous African" and "imported Islamic" ideas suggests two contrasting and internally coherent ideologies. The dichotomy fails to recognize that men and women created ideologies with the intellectual materials at hand; these ideologies were often internally inconsistent and always liable to revision and change. Also, the argument that masters and slaves in African slavery shared a cultural universe fails to take into account the relationship between power and ideology in *all* forms of slavery. Masters exerted power over slaves; if masters saw slavery in terms of kinship ideology, it is not surprising that slaves also drew on that ideology and tried to make its tenets serve their own ends. Slaves and masters contended over the precise meaning of kinship. Masters could regard it as strengthening hierarchy; slaves could regard it as ensuring security and benevolent treatment. Masters and slaves engaged in the same kind of contention in Islamic Africa. Islam provided an ideology supporting both the power of masters and the rights of slaves. The dichotomy separating "African slavery" from slavery in Islamic Africa thus obscures how any ideology of dominance became a ground for struggle between masters and slaves.²⁶

Lovejoy's own insights argue that Africans, whether Muslims or not, created

²⁵ Igor Kopytoff, "The Cultural Context of African Abolition," in Miers and Roberts, *End of Slavery in Africa*, 490–93.

²⁶ For analyses linking the ideology of slavery to struggles between masters and slaves on the East African coast, see Cooper, "Islam and Cultural Hegemony"; and Jonathon Glassman, "The Bondsman's New Clothes: The Contradictory Consciousness of Slave Resistance on the Swahili Coast," *J Afr H*, 32 (1991): 277–312. Like Kopytoff, Glassman recognizes the nuanced gradations of dependency in slavery, especially before the rise of plantation slavery, but he also stresses the multiplicity, and often contradictory meanings, of ideologies (see 277, 279–80, 282, 288–92, 310–12).

patterns of slavery whose similarities transcend his distinction between Islamic and kin-based societies. In parts of what became Atlantic Africa even before the impact of the Atlantic slave trade, slavery had already become an important "institution . . . related to more complex social structures than kinship." Something of the same process seems to have occurred in the Islamic world, including Islamic Africa, where Lovejoy indicates that slaves performed certain kinds of work because social structures were on a larger scale than kinship groups. Lovejoy also notes that "the emergence of Islamic slavery in Africa" was most closely duplicated by slavery in the Zambezi Valley in the eighteenth century.²⁷ In the nineteenth century, slaves became more important to local production, without necessarily strong links to the collapse of the export market, in parts of Islamic West Africa and non-Islamic Central Africa. To me, these intriguing comparisons argue that regions of Atlantic Africa and Islamic Africa were experiencing similar processes. But we obscure any similarity when we use distinctions between Islamic and kin-based societies, or Islamic and European external influences, to characterize different forms of slavery. We need ways of thinking about slavery and the slave trade that allow us better to consider both Islamic and non-Islamic Africa.

THE BOOKS UNDER REVIEW, including the two syntheses, suggest how we can begin to build a comparative framework for slavery and slave trades in both Atlantic and Islamic Africa. Including the entire sub-Saharan continent in a synthesis, given our current state of knowledge, impedes comparative work by subordinating Islamic Africa to an analysis developed for Atlantic Africa. Instead of focusing on Africa as a setting for slavery, we need to find other, more specific settings shared by slavery and the slave trades in both Islamic and Atlantic Africa. These books describe three comparable settings. The first and geographically widest was the regional commercial network, between parts of Africa and parts of the Americas or external Islamic world, that provided the transport and markets transforming human beings into commodities. The second setting was the state, whose violence often enslaved men, women, and children.²⁸ The third, and narrowest in scope, was the household in which many slaves—especially but not exclusively in the external Islamic world—found themselves working.

These settings provide a common, though certainly not exclusive, framework for all of Africa's slave exports and slavery within the entire sub-Saharan continent. Whether in the Islamic or Atlantic worlds, the dynamics common to slavery manifested themselves in these three settings. At the same time, each

²⁷ Lovejoy, *Transformations in Slavery*, 40, 41, 17, 133.

²⁸ Meillassoux and Curtin both analyze how warfare enslaved people and how traders exchanged commodities, including slaves. More than Curtin, Meillassoux puts military violence at the center of his analysis; more than Meillassoux, Curtin discusses trade networks whether or not slaves were among the trade commodities. See Claude Meillassoux, "Introduction," *The Development of Indigenous Trade and Markets in Africa* (London, 1971), 3–86; Meillassoux, "Female Slavery," 64; "Correspondence," 321, 322, 327; *L'anthropologie de l'esclavage*, esp. 43, 45–50, 141–234, 235–303; Philip D. Curtin, "Pre-colonial Trading Networks and Traders: The Diakhanke," in Meillassoux, *Development of Indigenous Trade and Markets*, 228–39; most of *Economic Change in Precolonial Africa*, vol. 1, devotes itself to exchange and production for exchange—but for Curtin's argument about political and economic models of violent enslavement, see esp. 156–57, 180–82.

setting reveals the distinctive nature of slavery in Islamic Africa and of Africa's slave trade to the external Islamic world. We can begin to understand whether and how slavery helped usher in regional economic transformations, how slaves performed important economic roles in the absence of plantations, why slavery appears so pervasive in Islamic Africa, and how slavery took shape in the presence of Islamic ideology.

Because no single network bound Africa to the external Islamic world, the first task is to distinguish various, often overlapping, regional commercial economies. Taking inspiration from Miller's *Way of Death*, we need to trace not only movements of commodities but also the flows of capital and credit that Clarence-Smith recognizes as "the biggest black hole" in our knowledge about the slave trade in the Indian Ocean.²⁹ *The Economics of the Indian Ocean Slave Trade* reveals the complexity of the commercial networks centered on the Indian Ocean and expanding into the Nile Valley, Red Sea, and Persian Gulf. In the eighteenth and nineteenth centuries, trade networks bound these regions more tightly than ever. In the Persian Gulf, described by Thomas Ricks in his essay "Slaves and Slave Traders in the Persian Gulf," both overland and maritime trade flourished, and merchants invested in production.³⁰ On the other side of the ocean, the East African island of Zanzibar became the seat of the vast mercantile empire analyzed in Abdul Sheriff's *Slaves, Spices and Ivory in Zanzibar*.

What kinds of transformations did this trade initiate? In East Africa, according to Sheriff, slavery was transformed in the pattern described by Lovejoy—slaves began to produce export commodities. But a transformed system of slavery failed to nurture a wider transformation in investment and production. Sheriff makes his argument with some reference to the Atlantic world. In both Atlantic Africa and the East African coast, the same processes transformed slavery. In the early nineteenth century, the end of the overseas slave trade—whether to the Americas or French Indian Ocean islands—intensified the exploitation of slaves within Africa. On the African coast and offshore islands of the Indian Ocean, Omani émigré merchants no longer exported slaves but put them to work on plantations growing cloves, whose export boomed between about 1810 and 1850. The earlier transformation of the Atlantic world prevented a similar transformation of the Indian Ocean basin. The African commercial frontier expanded on the wave of a favorable price structure that originated in industrialized Europe. European factories manufactured exports for such low prices, and the European middle classes bought African imports, especially ivory, at such high prices, that the most successful East African traders invested in foreign trade rather than production. Moreover, the Indian traders who financed much of the commercial expansion could not invest in production even if they had wanted to; as British subjects, their slaves had been confiscated by the local consul in 1860. As for the other side of the Indian Ocean, Sheriff argues that Persian Gulf economies did not rely on slaves; the region did not import large or growing numbers of slaves from East Africa.

Analyzing the nineteenth-century Persian Gulf, Ricks confronts one of the

²⁹ Clarence-Smith, *Economics of the Indian Ocean Slave Trade*, 11.

³⁰ Thomas Ricks, "Slaves and Slave Traders in the Persian Gulf, 18th and 19th Centuries: An Assessment," in Clarence-Smith, *Economics of the Indian Ocean Slave Trade*, 60–70.

conundrums of slavery in the Islamic world outside Africa: in the absence of institutions based on slavery, did slaves play an important economic role? Disagreeing with Sheriff, Ricks argues that slavery and the slave trade were central to the transformation of the Persian Gulf. In the nineteenth century, asserts Ricks, the region did indeed import increasing numbers of slaves. Profiting from the slave trade, Omani merchants became the financiers of the Gulf. Other merchants, as well as landowners and officials, bought slaves and exploited them in a wide range of agricultural, artisanal, maritime, military, and commercial activities. Masters and slaves did not create new institutions or forms of production based on slavery. Instead, slaves performed the vital function of filling temporary labor shortages in the rapidly expanding commercial system.

The pattern of slave labor described by Ricks arose because characteristics common to slavery manifested themselves in the particular context of the Persian Gulf. The apparent diffuseness of Gulf slavery, so unlike East African or American plantation slavery, highlights the essential characteristic of slavery as control over labor rather than as a particular form of labor. In the Americas and East African coast, control manifested itself in the work regime of the plantation. In the nineteenth-century Persian Gulf, as well as other parts of the Islamic world, control allowed masters to move workers quickly from one endeavor to another. As the need presented itself, slaves could be porters, dockworkers, servants, farm laborers, weavers, or pearl divers. Slave labor was mobile labor. And mobility assumed particular importance, perhaps, in places where other workers could not be easily or rapidly dislodged from their activities. Thus we see another central dynamic of slavery: the presence of other workers influenced where and how slaves labored. In Oman, slaves worked on date plantations; in Mesopotamia, attempts to make slaves produce dates failed. The failure resulted, according to A. Jwaideh and J. W. Cox in their essay in *The Economics of the Indian Ocean Slave Trade*, because tenant farmers possessed skills and commitment superior to those of slaves.³¹ Tenant farmers also might have successfully opposed attempts to replace them with slave labor. Paul Lovejoy's insight that slavery affected relations among all classes can be reversed. Relations between classes—landowners and tenants, for instance—shaped how and whether slaves were exploited.

The Africans who unwillingly crossed the eastern and western oceans, as well as the Sahara Desert, had become slaves by coercion, often, though not always, at the hands of captors. Because force of arms was a fundamental dynamic of enslavement, and because such force frequently took the form of kings' warriors, African states represent a second shared setting for slavery and the slave trade. In addition, African ruling elites sometimes exploited slaves who—as soldiers, farmers, or other workers—helped produce a surplus for the state.³² The

³¹ A. Jwaideh and J. W. Cox, "The Black Slaves of Turkish Arabia during the 19th Century," in Clarence-Smith, *Economics of the Indian Ocean Slave Trade*, 45–59, esp. 52.

³² In the Kongo kingdom, for example, slaves were essential to producing a surplus for the town nobility, because slave production could be controlled and concentrated more effectively than the production of rural peasants. John Thornton, *The Kingdom of Kongo: Civil War and Transition, 1641–1718* (Madison, Wis., 1983), 18, 20. As both Thornton and Frederick Cooper point out, slave production existed alongside other forms of surplus extraction. Thornton, 16–17, 23–24; Cooper, "Problem of Slavery," 116.

prevalence of slavery in the kingdoms of Islamic Africa thus resulted not from Islam alone but rather from processes specific to African states.³³

Does distinguishing African kingdoms as important settings for slavery and the slave trade overemphasize the significance of states? After all, armed men other than kings' warriors enslaved people (and natural disasters, such as famines, played a part); long-distance exchange sometimes flourished without the protection of centralized government; masters sometimes exploited slaves without calling on the coercive, judicial, or ideological backing of states. I believe it is still useful to focus attention on states for two reasons. First, forms of slave raiding, trading, and exploitation in the apparent absence of states sometimes resulted from the temporal or geographic proximity of states. Institutions of violence and commerce often undermined and outlived the very states that first nurtured them.³⁴ And states—especially predatory states—perhaps encouraged the elaboration of particular, complex forms of “stateless” organization on their frontiers.³⁵ Second, I am not arguing that slavery or the slave trade was exclusively the domain of states but that examining states provides one comparative bridge between Islamic and non-Islamic Africa.

Richard Roberts offers insights into how state power both stimulated and depended on slavery, and how Islamic ideology played a surprisingly ambiguous role in slavery, in his study of political economy in the middle Niger. *Warriors, Merchants, and Slaves* examines three states between the eighteenth and twentieth centuries: the kingdom of Segu, based on local ideologies but including Muslim subjects; the Islamic theocracy established by al-Hajj Umar; and the French colonial regime. In Segu, slavery underpinned state formation itself. The state arose when a warrior transformed a cohort of young, unmarried men into an armed band. This army gained new recruits by capturing slaves, who then themselves became warriors. As long as the king controlled his warriors, warfare sustained state power by producing the booty, including slaves, which could be either traded or consumed directly by the elite. But uncontrolled warfare—warriors making raids on their own people—turned inward and threatened the kingdom.³⁶

³³ Lovejoy and Manning both hint at the importance of state structures in shaping slavery in the Islamic world, including Islamic Africa. Lovejoy, for example, notes the “larger scale” of Islamic societies; *Transformations in Slavery*, 17; it is also in the context of states that he makes his argument that “indigenous developments were more important than external influences” in the rise of a slave mode of production on the African Islamic frontier between 1400 and 1600; 27–28, 31–33. Manning cites the work of J. P. Olivier de Sardan, suggesting that the needs of the monarchy resulted in slavery in the Islamic kingdom of Songhai; *Slavery and African Life*, 128. Olivier de Sardan's essay appeared in Meillassoux, *L'esclavage en Afrique précoloniale*, 99–134.

³⁴ For commercial institutions, see Meillassoux, *L'anthropologie de l'esclavage*, 56–57. For a detailed discussion of how merchants undermined the authority of kings, who had previously patronized and controlled commerce in a pre-colonial Sudanese state, see Jay Spaulding, *The Heroic Age in Sinnar* (East Lansing, Mich., 1985), xiii–xiv, 112–19, 139–49, 176–87. In the Turkish-ruled Sudan, slave raiding was initiated by the armies of the conquest government, then taken over by merchants. These private armies, violent and highly competitive, eventually undermined Turkish rule. See Janet J. Ewald, *Soldiers, Traders and Slaves: State Formation and Economic Transformation in the Greater Nile Valley, 1700–1885* (Madison, Wis., 1990), 163–77.

³⁵ Peter P. Ekeh, “Social Anthropology and Two Contrasting Uses of Tribalism in Africa,” *Comparative Studies in Society and History*, 32 (1990): 680–83.

³⁶ Roberts explicitly draws on Meillassoux in his account of state formation; Richard L. Roberts,

Slavery not only underpinned state formation, but political processes then transformed slavery. Rulers and their warriors gathered resources only to turn them over to other owners, whether in Islamic or Atlantic Africa.³⁷ On the fringes of Atlantic Africa, in what is now Botswana, masters gained slaves as part of the politics of the Ngwato kingdom. During the last half of the nineteenth century, as Michael Crowder and Suzanne Miers relate in their essay "The Politics of Slavery in Bechuanaland," the Ngwato kings expanded into new hunting and herding grounds and claimed the labor of the conquered Sarwa inhabitants. A later king, however, ceded to notables, clients, and even ordinary subjects his royal prerogatives over Sarwa labor. No longer collectively slaves of the king, Sarwa became slaves of individual Ngwato owners.³⁸ In Segu, according to Roberts, new ways of exploiting slaves arose from state activities. Frontier warfare provided both security in the core of the kingdom and slaves for the market. Muslim merchants bought slaves and put them to work on plantations of grain and indigo. By the nineteenth century, the slave-owning merchants of Segu formed a class of urban masters who depended on their agrarian holdings and slave laborers.

Because states invoked ideologies, an examination of states illuminates the relationship between Islamic ideology and slavery. Here, Islam indeed made a difference. Recognizing and enforcing Islamic law, Muslim rulers legitimated the enslavement of non-Muslims and defined the obligations of masters over slaves. But commitment to Islam did not always encourage slavery. Islamic ideology dealt with much more than slavery, and Muslims acted on their beliefs in a multitude of situations. In the Islamic theocracy that conquered Segu, reveals Roberts, Muslim rulers seeking to fulfill the law and reinforce their power unintentionally dismantled the slave plantations in the core of the state. Like the Segu kingdom, al-Hajj Umar's state was a warrior state. But, Roberts argues, while Segu's warfare stimulated economic growth and transformation, Umarian warfare led to economic collapse. At the center of the contrast lay ideology. Islamic ideology dictated both a certain distribution of war booty and taxation of subjects. Trying to follow these prescriptions, Muslim leaders alienated merchants and failed to create a cohesive army. The middle Niger disintegrated into mutual raiding and rebellion. Some slave owners made their way to the desert edge where, by the 1870s, they had established flourishing plantations and towns beyond the striking range of the Umarian state. Islamic theocracy, in a sense, impelled the expansion of slavery. But slavery expanded because Muslim merchants sought to escape the instability and demands of the Islamic state.

Slavery in the third setting, the household, has not received the same attention from scholars as plantation slavery. We have tended to assume that domestic or

Warriors, Merchants, and Slaves: The State and the Economy in the Middle Niger Valley, 1700–1914 (Stanford, Calif., 1987), 30–31; his argument about warriors and slaves—that warriors produced slaves—is also reminiscent of Meillassoux. Roberts uses this argument about the political economy of slave raiding to revise Curtin's models of political and economic models of enslavement; p. 37.

³⁷ For an analysis of the political dynamics forcing rulers to yield land and labor to private owners in the greater Nile valley, see Ewald, *Soldiers, Traders, and Slaves*, 145–58, 166–67, 186–87.

³⁸ Michael Crowder and Suzanne Miers, "The Politics of Slavery in Bechuanaland: Power Struggles and the Plight of the Basarwa in the Bamangwato Reserve, 1926–1940," in Miers and Roberts, *End of Slavery in Africa*, 172–200, see esp. 175–76.

household slaves filled social more than economic functions; perhaps we have also assumed that such social functions were less liable to change than economic functions and thus less amenable to historical analysis. For the Islamic world outside sub-Saharan Africa, in particular, the image has persisted of household slaves—concubines, eunuchs, servants—playing not economic but merely social roles. And these supposedly arose from ideals of domestic and family life that were particularly Islamic. But, Muslims or not, masters owned slaves as part of an entire configuration of household resources. How masters evaluated the range of these resources, according to two authors, determined how they exploited slaves.

In both Christian Ethiopia and Muslim Mesopotamia, a household head's decision to use—or not to use—slaves depended on how rights over land were defined. The Lasta households of late nineteenth-century northern Ethiopia, analyzed by James McCann in “‘Children of the House,’” present an apparently anomalous situation.³⁹ Even in a period of economic depression, when households did not profit from selling their produce, masters put slaves to work in the fields. But these masters made their choice on a sound economic basis. The land tenure system made the labor of both wage workers and family members dangerously unreliable. Free people could exercise rights to land. As soon as a hired hand or son saw an opportunity for establishing his own farm, he left the household. But slaves could not claim land. For this reason, household heads preferred slaves over family or wage labor for agricultural work. Slaves thus performed an essential economic function for Lasta household heads, ensuring a supply of labor rather than necessarily producing crops for the market. The rights of households to land had a very different impact on slavery in part of the Muslim world. Among the rural and village beduin of Mesopotamia, according to Jwaideh and Cox in *The Economics of the Indian Ocean Slave Trade*, membership in a tribe gave households their rights to cultivate land. Working the land thus denoted belonging to a tribe, which in turn marked an individual as free. If slaves labored in fields, they would have attained the hallmark of freedom—working the land and thus tribal membership. For this reason, masters refused to use slaves for agriculture.

Jwaideh and Cox also enrich our understanding of law, ideology, and slavery in the Islamic world. Patterns of slave exploitation among the beduin—slaves did not perform agricultural labor, for instance—resulted from notions about rights to land, tribal membership, and freedom. But these prescriptions derived from specifically beduin ideology and not Islam. As Roberts showed us that Islamic ideology had contradictory influences on slavery, Jwaideh and Cox reveal that the Islamic world itself did not adhere to any single ideology of slavery and freedom. Instead, they argue, we need to examine slavery in terms of the varied cultures within the Islamic world.

The cultural norms supposedly underlying household slavery in the Islamic world not only differed from one Muslim society to another but also may have changed through time. The eighteenth and nineteenth-century growth of commerce perhaps encouraged masters both to stress the privacy of their households

³⁹ James McCann, “‘Children of the House’: Slavery and Its Suppression in Lasta, Northern Ethiopia, 1915–1935,” in Miers and Roberts, *End of Slavery in Africa*, 332–61.

and to claim slaves as household members. Here we can speculate about the analysis of Jwaideh and Cox, revising and extending it. When they write about townspeople, the two authors repeat the common argument that Muslim urban households kept slaves in order to preserve privacy. The household's windowless walls bespoke a private realm, "unaccountable to [the world] outside its confines."⁴⁰ Part of accountability to the public world might have been yielding resources to the state, usually in the form of taxes. Merchants and others who were accumulating capital perhaps sought to lodge their wealth, including their slaves, in the "private" realm of the household, protecting it from the demands of the state. In this historical context—the growth of a commercial middle class—slave masters may have staunchly defended the status of slaves as household members and the status of the household as beyond the reach of outside interference.

Nowhere in this do we yet hear clearly the voices of men and women caught up in slavery in Islamic Africa or Africa's slave trade to the external Islamic world. Why are there no voices like those of Atlantic Africa, telling us about bodies sprouting cowrie shells or rituals to heal afflicted slave traders? It is true that in Islamic ideology, a body of beliefs that recognized slavery and the slave trade, no metaphors of evil like those of Atlantic Africa arose. But we should not attribute too much power to a supposedly monolithic ideology. Moreover, the view that Islam, more than any other force, shaped the ideals and practice of slavery is perhaps itself partly the creation of the particular historical context of the ending of slavery. It is possible that the dialogue that took place at the time between Western observers and Muslim slave masters reinforced the notion that Islam both sanctioned slavery and supported a certain form of slavery. The end of slavery was an era not only when slaves found freedom, of one sort or another, but also when Europeans, Africans, and Asians forged new identities and ideologies.

IN BOTH ATLANTIC AND ISLAMIC AFRICA, the transformation of slavery reached its height just before colonial conquest. The colonial conquerors regarded slavery as central to their endeavor. Part of the "civilizing mission," as they saw it, was to end slavery. Indeed, by defeating militant African states, colonial armies helped end the slave trade. But ending slavery itself was more complicated. *The End of Slavery in Africa* delineates the series of dilemmas faced by colonial administrators. Officially committed to abolition, they needed both goods and services supplied by slaves. They were usually underfinanced and understaffed, and they feared antagonizing their African elite allies and arousing the disorder that they believed emancipation would create. Always full of ironies, colonial governments were at their most incongruous as they grappled with the dilemmas of ending slavery. Throughout Africa, colonial rule in its early years sometimes unintentionally encouraged slavery and sometimes simply replaced slavery with other extremely coercive labor systems.

⁴⁰ Jwaideh and Cox, "The Black Slaves of Turkish Arabia during the 19th Century," 52.

Yet slavery ended in Africa. Slaves ending their own servitude are the central historical actors of many of the essays in *The End of Slavery in Africa*, as well as *Warriors, Merchants, and Slaves* and the two syntheses. In Manning's words, the "freeing of slaves was primarily an achievement of the slaves themselves." This is not to say that colonial ideologies and colonial economies played a minor role, or that ex-slaves enjoyed an unambiguous freedom. Slaves maneuvered for their freedom—however they envisaged that freedom—in an environment that colonial regimes helped establish, though often unintentionally. The new colonial political economies had the greatest impact on how slaves freed themselves and what freedom meant. Ex-slaves, in turn, helped shape colonial economies by laboring, or refusing to labor, as workers and peasants. Freed men and women also influenced the households of their former masters. To replace slaves, masters turned to their own families. Wives and children, Richard Roberts writes, became the new "captive labor."⁴¹ The end of slavery did not so much break relationships between masters and slaves as engage Africans—as well as Africans and Europeans—in new confrontations.

In the confrontations attending the end of slavery, men and women created new identities and ideologies. Racist ideology, argues Manning, assumed special virulence when slavery ended and former masters confronted free African and African-American workers. For their part, Manning maintains, Africans and African Americans responded to racism, as well as to the shared tragedy of slavery and the slave trade, by finding a common, pan-African identity. If we apply Manning's analysis to the process described by Roberts, it seems possible that African household heads who had lost their slaves tried to rework ideologies of kinship and domestic obligations in order to control their new "captive labor." Ideologies of resistance to colonialism also emerged from the experience of slavery. Emancipation from slavery supplied a powerful metaphor for Africans seeking freedom from colonial masters. The Ganda, according to Michael Twaddle in his essay "The Ending of Slavery in Buganda," expressed their opposition to the British in terms of resistance to slavery: "we will not make bricks for the Europeans." Just before the onset of formal colonial rule, relates Lovejoy, slaves on the East African coast rebelled against their Muslim masters. From their fortification, they flew a white flag with the word "freedom" written on it in Swahili. Did Africans first rally under the banner of *uhuru*, a word that came to mean much during the anticolonial struggle, as slaves seeking their freedom?⁴²

Ideas about the shaping of slavery by Islamic ideology also emerged from confrontations over the end of slavery. The gradual freeing of slaves in northern Nigeria is explored by Jan Hogendorn and Paul Lovejoy in "The Reform of Slavery in Early Colonial Northern Nigeria," which analyzes the interactions among Muslim slave masters, slaves, and British officials. Sir Frederick Lugard, the first British high commissioner, shared a common ideological stance with

⁴¹ Manning, *Slavery and African Life*, 16; Roberts, *Warriors, Merchants, and Slaves*, 199.

⁴² Michael Twaddle, "The Ending of Slavery in Buganda," in Miers and Roberts, *End of Slavery in Africa*, 137. Jonathon Glassman and Steven Feierman indicate that people in East Africa developed an understanding of *uhuru* specifically out of the experience of slavery and dependency. Glassman, "Bondsman's New Clothes," 309; and Steven Feierman, *Peasant Intellectuals: Anthropology and History in Tanzania* (Madison, Wis., 1990), 212–14.

Muslim masters. Both Lugard and his elite subjects regarded slavery as sanctioned by Islamic law. For Lugard, this religious sanction meant that slavery could not be abolished abruptly without disrupting the entire fabric of an Islamic society, but slavery could be ended by employing the institutions of Islam itself. In fact, the British administration used local Islamic law to enable slaves to buy their freedom. At the same time, the colonial political economy—specifically, its tax regime—made slavery costly and manumission cheap, thereby encouraging masters to free their slaves. British rule encouraged masters to fulfill one aspect of Islamic ideology: the prescription for paternalistic benevolence, for charitable grants of manumission. As Hogendorn and Lovejoy remark, “the ideal of slavery under the caliphate [the pre-colonial Islamic theocracy] . . . came close to being the reality of slavery under colonialism.”⁴³ The British, by regarding slavery as Islamic, strengthened the concept of Islamic slavery even as slavery ended.

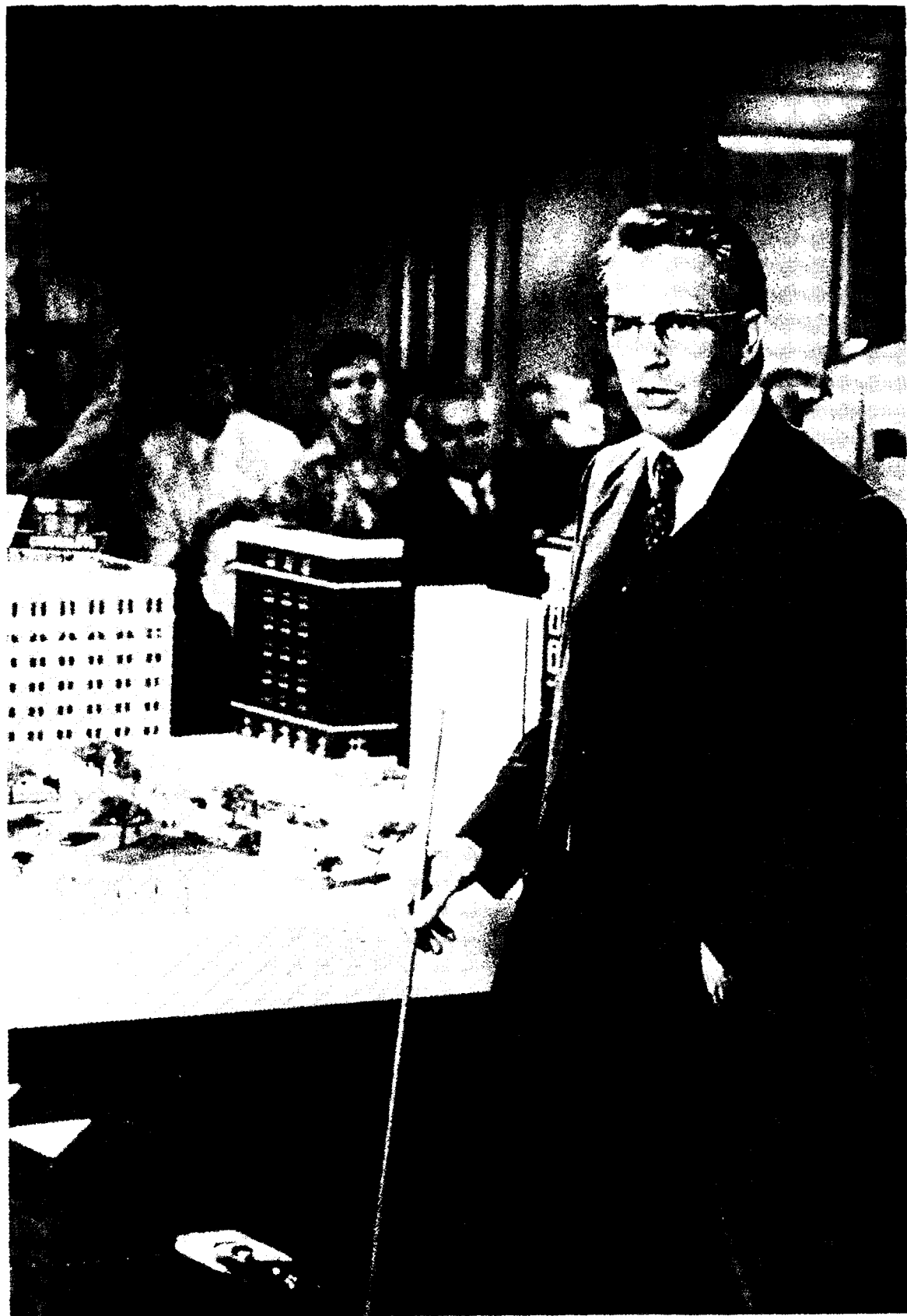
The example of northern Nigeria helps us trace the origins of one Western approach to slavery in the Islamic world. Historians sometimes speak of “Islamic slavery” as if it were one homogeneous set of institutions bearing distinctive characteristics that can be explained by reference to a religion. It is possible that, in the last half of the nineteenth century, Muslim masters and Europeans unintentionally collaborated to strengthen this concept of “Islamic slavery.” Challenged by Europeans, Muslim masters could have invoked the sanctity of their religion and their households to justify their control over slaves. As slaves became more of a economic liability than an advantage, masters enacted the paternalistic strand of Islamic ideology, manumitting their slaves but often binding the freedmen to them by clientage. The Muslim masters’ version of “Islamic slavery” likely found a ready audience among European officials who faced the dilemma of pitting emancipation against disorder. The dilemma was avoided, if not resolved, by regarding slavery as peculiarly “Islamic.” An intractable institution was simply one part of an equally intractable religion. To force change would be to spark fanaticism. For their part, some Muslims—such as the Somali clan leaders mentioned by Lee Cassanelli in his essay “The Ending of Slavery in Italian Somalia: Liberty and the Control of Labor, 1890–1935”—nurtured European images of “Muslim fanatics” by simultaneously calling on Islamic ideology and making opposition to slavery part of their resistance to colonialism.⁴⁴

More distant origins of our image of “Islamic slavery” go back at least to the eighteenth century, when westerners questioned the place of slavery in an age of revolution. Europeans increasingly regarded the values of the West as progressive and democratic in opposition to an Islamic East, which was supposedly backward, stagnant, and despotic. And all of these “Islamic” characteristics manifested themselves in slavery. Thus the same intellectual currents that increasingly articulated slavery as a contradiction and problem for the West also accepted slavery as intrinsic and thus unproblematic for the Islamic world. What endowed

⁴³ Jan Hogendorn and Paul Lovejoy, “The Reform of Slavery in Early Colonial Northern Nigeria,” in Miers and Roberts, *End of Slavery in Africa*, 395.

⁴⁴ Lee Cassanelli, “The Ending of Slavery in Italian Somalia: Liberty and the Control of Labor, 1890–1935,” in Miers and Roberts, *End of Slavery in Africa*, 319.

studies of slavery in the Atlantic world with a rich inheritance left scholars of slavery in the Islamic world with a much poorer legacy. Reclaiming an intellectual heritage, we must problematize slavery in the presence of Islam. And problematizing slavery means bringing it into the realm of historical change and contingency. The books under review help us do this. Especially when read together, they begin to show us how slavery in Islamic Africa and Africa's slave trade to the external Islamic world shared the dynamics and ironies of slavery elsewhere.



AHR Forum
JFK and the Culture of Violence

MARCUS RASKIN

THE FILM *JFK*, BY OLIVER STONE and his colleagues, has had an extraordinary effect on the public consciousness. In a few months' time, the film has generated concrete political actions that never would have occurred had the film not been made or had it not struck the chord of reevaluation that comes at the end of a war/Cold War period. *JFK* seems destined to lead to the opening of the hundreds of thousands of papers collected by the House Select Committee on Assassinations and the Warren Commission but now under seal. (Perhaps this time, the government will even index the papers and hearings, something that neither the House Committee nor the Warren Commission did.) The reason for the film's effect is that it is powerful cinematically. Contrary to what some would like to believe, it is surprisingly accurate. On the complex question of the Kennedy assassination itself, the film holds its own against the Warren Commission. The speculations that various characters spout are too broad brush. Even so, the seeds of further inquiry are planted.

As a work of art, *JFK* succeeds because it confronts powerful emotions and political truths that are as age-old as Homer and Sophocles. It does no good to pick apart the rendering of an event by an artist. His or her purpose is not the particular but the general. It is to take an event and see within it a series of truths, some felt, some unconsciously understood and hardly articulated, that make sense and meaning of an event, its cause, and its implications. Indeed, artists-dramatists dare to present through a book, drama, painting, or film the structure and moral character of an entire age, which necessarily includes its agonies and foibles. Some of these explanations are tendentious, silly, paranoid, vengeful, scapegoat-oriented, and sheer lies. But *JFK* cannot be dismissed this way, for it is not a lie. It is a myth of heroic dramatic proportions that "is true precisely because it has happened so many times that it must be retold again and again to explore the dimensions and varieties of its truth."¹

The Report of the Warren Commission had a different purpose. It was ostensibly concerned with facts, although that concern was secondary to using the language and structure of conservative authority to move the nation from dis-ease to ease about the events of the Kennedy assassination. Stone's *JFK* has a filmic political objective in the literary genre of Theodore Dreiser: to be disruptive

¹ Richard McKeon on Thomas Mann, *Thought, Action, and Passion* (Chicago, 1954), 226.

ostensibly for the purpose of getting at the truth of the American government. Still in literary terms, the Warren Commission's political purpose was closer to that of Herman Wouk's establishmentarian novels. The commission's final report was intended to soothe those who had doubts. This connection is noted by Kai Bird in *The Chairman*, his new book on John J. McCloy, the distinguished establishmentarian who was a member of the Warren Commission. McCloy stated that the commission had to be unanimous even though three of the members who held elective office, Richard Russell, Hale Boggs, and John Sherman Cooper, had grave doubts about the single bullet theory and the notion that there was no conspiracy. These doubts were also pointed out by Edward Jay Epstein in his early book on the Kennedy assassination, *Inquest*. McCloy believed that it was time to assuage the nation, let the dust settle over the dastardly events and move forward. Thus he wrote language, carefully crafted indeed, that would allow healing and soothing to work itself into the body politic, and would rally all members of the commission to sign on, their doubts notwithstanding.² It is not difficult to understand why the commission sought to quiet people's questions, however misplaced its intentions seem today.

The assassination occurred approximately one year after the Cuban missile crisis, a period in which people had been treated to the strong possibility of nuclear war. It seemed a continuing imminent threat. Kennedy's assassination added greatly to fears of instability and world crisis, and doubts about the character of American governance spread immediately to Europe. As Harrison Salisbury of the *New York Times* stated in his introduction to an edition of the Warren Commission report to which he, Anthony Lewis, Tom Wicker, and James Reston (all of the *Times*) contributed, "Not infrequently (such) groups (the 'Who Killed Kennedy committee' which included Bertrand Russell, Lord Boyd-Orr, Sir Compton Mackenzie, J. B. Priestley, Professor Hugh Trevor-Roper, Kingsley Martin and Michael Foot) . . . compare the Kennedy killing to the Dreyfus affair—the inference being that the whole weight of authority of the American Establishment—Government, Big Business, the Power Structure of Society—has been placed behind a campaign to rest the blame on a single (presumably innocent) man."³

FOR AN ENTIRE GENERATION, the scar over the healing process of forgetfulness about the Kennedy assassination hid a festering sore of doubt. This sore on the body politic spread as a result of the Indochina war, secret wars from Angola to Cambodia, assassination plots the United States participated in or initiated, the CIA's involvement in the sale and growth of heroin and opium as well as its experimentation with LSD on unwitting subjects, the use of covert agents and assets to intervene directly in the American political process, FBI illegal wiretaps and black-bag jobs, bribes to foreign leaders, and harassment of black minorities.

² Kai Bird, *The Chairman* (New York, 1992); Edward Jay Epstein, *Inquest: The Warren Commission and the Establishment of Truth*, Richard H. Rovere, intro. (New York, 1966).

³ United States Warren Commission, *Report of the Warren Commission on the Assassination of President Kennedy*, Harrison E. Salisbury, intro. (New York, 1964), xxv.

Many of these governmental activities were hidden behind the veil of secrecy and loyalty oaths, which warded off independent scholars, politicians, or mere citizens from looking too carefully at how the national security state actually operated. National security leaders used half-truths, lies, and plausible deniability in order to mask the real state of affairs. And then there were the doubts raised by the Warren Commission itself.

The proofs the commission offered grew more dubious with the passage of time. There was a single bullet that supposedly passed through Kennedy and Governor John Connally. Discovered on Connally's stretcher at Parkland Hospital, it was virtually pristine, an incredible possibility given what it had struck. The commission relied on the FBI and CIA. It had no way of independently verifying what these agencies told it. What has come to be known about these agencies since that time has only increased doubts about the commission's findings, which seemed designed to protect covert activities such as gun running to Cuba and CIA involvement with gangsters.

Nor did the commission follow what is called the best evidence rule in reconstructing the assassination. To do so, it would have had to replicate as closely as possible the conditions of that day to see whether another marksman could have successfully hit a moving target in the position the president was seated. The FBI made clear that they tested a rifle that did not replicate the one fired under the conditions of November 22.⁴ Thus, as I have argued elsewhere, "The Commission now credits Oswald with doing extraordinary things without showing that one man could do them."⁵ The president's commission should have reconstructed the events by having an ex-marine of Lee Harvey Oswald's approximate background, physical size, and marksman ability see whether he could re-create Oswald's alleged feat of marksmanship. The commission might then have asked the ex-marine to perform within a 43-minute period Oswald's supposed subsequent activities. Between 12:33 and 1:16 p.m., Oswald is alleged to have shot the president and Governor Connally, left the School Book Depository (from the sixth floor), taken a "7 block walk on Elm Street, a bus ride toward the area he had just left, another walk to his rooming house where he spent 3 or 4 minutes, a pause at a bus stop for an unspecified length of time, a walk almost a mile long to the intersection at East 10th Street and Patton Avenue, and at last, the confrontation and murder of Officer Tippit."⁶ If the best evidence rule was not followed, neither was anything like the falsifiability program for testing scientific hypotheses used to prove Oswald the lone assassin.

A generation grown to maturity in the 1960s later took it for granted that governments would and did lie. In the Cold War period, it did not take a feverish mind or great logicians such as Bertrand Russell to conclude that there was something rotten in the United States. But this conclusion did not come easy. Generations of journalists and academics had been educated in institutions of higher learning to the Platonic idea of golden lies, which guardians of the state

⁴ Testimony by FBI agent Lyndal L. Shaneyfelt, Warren Commission Hearings, vol. 5, 146.

⁵ Marcus Raskin, *Yale Law Journal*, 76 (1967): 567.

⁶ Mark Lane, *Rush to Judgment: A Critique of the Warren Commission's Inquiry into the Murders of President John F. Kennedy, Officer J. D. Tippit, and Lee Harvey Oswald*, Hugh Trevor-Roper, intro. (New York, 1966), quoted in Marcus Raskin, review, *Yale Law Journal*, 76 (1967): 568.

had an obligation to tell the lesser classes. The public was to be “educated” with “sanitized” stories about reality; it was a beast to be manipulated. From J. B. Watson to Walter Lippmann to Harold Lasswell, advertising, symbol manipulation, and propaganda were assumed to be necessary attributes to governing and consuming in a mass society. Organization men and experts were socialized to interpret the world to laymen according to a preexisting framework that denied the possibility of a “free gaze” regarding evidence. The Warren Commission’s bright young staff of lawyers were no match for its putative investigative arms, the CIA and FBI. Indeed, the idea of deeply questioning these organizations did not cross their intellectual radar screen, nor could they do so as long as Allen Dulles, the former head of the CIA, was on the commission. He had been fired by President Kennedy for the Bay of Pigs fiasco. In the aftermath of the Bay of Pigs, Howard Hunt became an adviser to Dulles for the purpose of defending Dulles and CIA covert operators against the New Frontiersmen, especially Robert Kennedy and Maxwell Taylor, charged by the president with assuring that such a failure would never occur again.

The war in Indochina shattered the secret establishmentarian/conventionalist way of doing business. Those directly victimized by that conflict began wondering about the character of the American government. Some came to believe that if John Kennedy had lived and had won a second term, the politics of America would have been much different and the nation would not have passed through the Indochina agony. Thus *JFK* seems to be the revenge of Oliver Stone’s generation. In Freudian terms, for Stone President Kennedy is transformed into an imago who would have warded off the evil and difficulties his generation and others passed through. Stone uses his imago, Kennedy, and his dramatic instrument, Garrison, to speak to the next generation, one that knows little American history. It receives its moral, political, and historical understanding about the past through images. Thus *JFK* is potential dynamite—a 40 million-dollar Hollywood version of a samizdat—for it has shaken a carefully constructed *Weltanschauung* that sought to teach the lesson that accidents and random events are more important in the processes of social, economic, and political life than structures and organization. *JFK* is meant to use the assassination to force an audience to decide whether it wants to ground the American political process in the post-Cold War era with the same structures and habits of mind that governed it during the Cold War. Should, for example, we continue to have secrecy in government obscuring our understanding through the opaque shield of state security?

SECRETS, OF COURSE, GIVE RISE TO PARANOIA, for they leave people feeling incomplete and used. To fatalists, the world may be nothing more than a series of random events and accidents, but most people crave a coherent explanation of why the events that shape their destiny occur. Indeed, this is a psychological function of history. Without this grounding, a person feels uneasy and unable to shape at least part of his or her destiny. Historians attempt to trace causes, people,

and events that come together. And so it is with political matters. It is true that many events in a person's life, as well as great historical events, appear to be accidental or random; they seem to have no explanation. Nevertheless, if one looks closely, rejecting the culture of violence and secrecy, an explanation and a cause may be found.

There is nothing random about an assassination nor is there much that is random about a state murder. It takes planning, steely nerves, killing ability, and a motive. It will almost always involve more than one person. Those who think that groups of people do not get together to bring about a particular result are surely out of touch with reality. People, and especially governments, act with will and intention. That their means may be illegal, even criminal, or that they may fail in their objective, does not change the irreducible fact that government officials get together to bring about a particular result. Indeed, this is what a conspiracy is when a criminal purpose is added to the definition.

Conspiracy is an activity that can be carried on by governments or by members within governments who are on a frolic or who are rogues. And, as numerous prosecutions by the state have shown, conspiracies are also carried out by some citizens. It is absurd to argue that conspiracies do not exist or that will and intention have given way to Gidean gratuitous acts that have no intention or explanation by the performer of the deed, the victim, or others examining the act. Thus it is far better and more accurate to begin from the assumption that conspiracies are common, especially in politics. As I have suggested, their objectives are criminal or illegal in either execution and result or both. This is why the Report of the Warren Commission, like most government documents, should be read from the recommendations through to the body, for, in the conclusions, the reader may begin to assess what is really bothering the writers: "The Commission believes that both the FBI and the Secret Service have too narrowly construed their respective responsibilities. The Commission has the impression that too much emphasis is placed by both on the investigation of specific threats by individuals and not enough on dangers from other sources."⁷

During the Cold War, governmental conspiracies violated international law or comity. For example, the attempt by the Kennedy administration to assassinate Castro and destabilize the Cuban economy, or to hire well-known criminals to kill Castro, was surely a criminal project. A prosecutor would have had little trouble bringing an indictment for criminal conspiracy. In whatever category we wish to place Operation Mongoose, we now know that a group of men did get together over a relatively long period within the American government to plot Castro's downfall. To Castro, this could have been understood as a conspiracy and an act of war. From the point of view of those committed to getting rid of the Cuban bone in the throat of American presidents, Operation Mongoose and the attempted assassination of Castro might have been understood as the height of good government and an effective use of American power.⁸ It should hardly

⁷ *Report of the Warren Commission*, 434–35.

⁸ Note how the same style continues. In February of 1992, the present head of the CIA was in the Middle East attempting to find ways of deposing and assassinating Saddam Hussein. A few years ago,

come as a surprise that real personal consequences might have followed for President Kennedy or his brother, Attorney General Robert Kennedy. Indeed, in a Senate Intelligence Committee report of 1975, former Senator Richard Schweiker concluded that pro-Castro Cubans killed Kennedy because of the attempts made on Castro's life. Castro continues to deny the charge and genuflects before Kennedy's memory.

The idea of Soviet involvement in the assassination, a fear that President Johnson expressed, was dissolved by the Warren Commission and Soviet KGB agents who later defected to the United States. No one wanted the assassination of a president to become a *casus belli* for nuclear war, least of all the establishments of the Soviet Union and the United States. Increasing its hold on the popular consciousness, however, is the story that the Kennedy assassination was a classic state murder to be analyzed by means of the "who benefits?" platitude of vulgar political science. *JFK* posits a massive conspiracy within the government and outside of it. The conspirators worked together to assassinate the president. The film insinuates that Johnson had criminal knowledge of what would happen to Kennedy if he visited Dallas and, furthermore, claims that Johnson paid off members of the military-industrial complex with a war in Indochina. Two other possible conspiracies are presented but given less weight, namely that an anti-Castro group killed Kennedy or that a Mafia group killed him. Indeed, the House Committee that investigated the assassination of President Kennedy gave credence to the idea that organized crime was involved in his assassination. Apparently, there are illegal wiretaps that support this theory. Robert Kennedy's Justice Department had carried on a campaign against organized crime with the president's blessing. The Teamsters, especially Jimmy Hoffa, hated the Kennedys. Hoffa believed that the Kennedy campaign had unleashed a vendetta against the Teamsters as a way to get to the White House. Hoffa and organized crime had ample reason to get even. Jack Ruby had numerous connections to the Mob that were not adequately explored at the time, according to a number of analysts of the Warren Commission report, including David Scheim in *Contract on America*. With many others (starting with Penn Jones and including Oliver Stone), Scheim points out that the inordinate number of witnesses who were murdered or died in suspicious circumstances are classic examples of Mafia involvement.⁹ Of course, a murder caused by organized crime may show the integral connections between crime and politics, but it does not rise to proportions that would cause fear and dread about the political system itself.

THE THEORY I PRESENTED IN A *Yale Law Journal* review of Mark Lane's important book, *Rush to Judgment*, embraced the idea of a possible conspiracy initiated by a group of anti-Castro Cubans who had been assets of the CIA but who had slipped

the government claimed that the Libyans had sent terrorist gangs to kill President Reagan, and in April 1986 Reagan sought to assassinate Muammar Qadhafi through massive bombing of his bunker.

⁹ David E. Scheim, *Contract on America: The Mafia Murders of John and Robert Kennedy* (Silver Spring, Md., 1983), chap. 3, p. 50 and following.

out of the CIA's control. In 1963, the Cuban exile community harbored great anger against President Kennedy. Various groups unauthorized by the CIA had been picked up and stopped from carrying out covert activities against Cuba. They had continued raids after the short-lived *détente* that developed between the United States and the Soviets after the Cuban missile crisis.¹⁰ And the CIA was very much involved with the Cuban exile community. Its largest station was in Miami in 1963, and it supported a wide variety of groups with weapons, money, and "technical assistance." There was, however, a political process of "simulopts" that was followed during the Kennedy administration. Simultaneous and contradictory policy options were pursued in order to see which would bear fruit. On the same day, November 22, 1963, that President Kennedy sent a message through the French journalist Jean Daniel to Castro that the United States wanted peace with Cuba, the CIA's Desmond Fitzgerald was negotiating in Paris with an assassin about killing Castro with a poison-tipped pen.

It was taken for granted in the CIA and Cuban exile community that a *démarche* had occurred in American policy the year before, after the Cuban missile crisis of October–November 1962. This change in fundamental policy split the CIA, for there were many such as Howard Hunt, a very active operative, who felt that Kennedy had sold out the Cuban exile movement and its attempt to destroy the Castro government at the Bay of Pigs and thereafter. He hated the New Frontiersmen who, he thought, were besmirching the good name of Allen Dulles and the CIA. Moreover, Kennedy's June 10, 1963, speech at American University called for an end to the Cold War as well as general and complete disarmament. After the speech, and within a month, the United States, through Averell Harriman and Carl Kaysen, signed an agreement to ban nuclear testing in the atmosphere, in space, and under water. In order to obtain support from the Joint Chiefs, Kennedy found himself having to agree to a hugely expanded underground nuclear testing program. This was also the only way that Kennedy was able to secure the support of both conservatives and national security liberals in the Senate such as Everett Dirksen and Henry Jackson.

In assessing Kennedy's record on national security and the effect it might or might not have had on his own assassination, it is important to remember that he signed his name to budgets and doctrines that caused the defense budget to jump within two and a half years from 39.5 billion dollars to approximately 52 billion. Former President Eisenhower complained bitterly about this increase and stated publicly that the Kennedy budget was wasteful, having virtually nothing to do with defense. In 1960, however, Kennedy had campaigned on the idea of flexible response and closing the missile gap. Of course, the missile gap of the early 1960s turned out to be the opposite of what was believed. The Soviets had between three and six liquid-fuel, long-range missiles, which could be easily spotted because of the relatively long lead time necessary to get them ready for firing, yet the United States continued to build huge numbers of missiles even after intelligence revealed that the Soviets had very few. Secretary McNamara also adopted a counterforce strategy in the early years, which became the ideological reason for

¹⁰ Raskin, *Yale Law Journal*, 579 and following.

continuing to arm on the nuclear level at a furious pace. But McNamara's position on the size of the arsenal was far less than the requests made by the Joint Chiefs in 1961–1962.

The second major shift from the Eisenhower administration, Kennedy's decision to accept the idea of flexible response, meant that the United States would not only fight limited and nuclear wars but that it would also challenge local revolutions and wars of national liberation to which Premier Khrushchev had given some support, both rhetorical and real. McNamara made the following request, summing up the Kennedy administration's defense policy before the House Armed Services Committee in 1964 as it related to the arms build-up:

A 100 percent increase in the number of nuclear weapons available in the strategic alert forces.

A 45 percent increase in the number of combat-ready army divisions.

A one third increase in the number of tactical fighter squadrons.

A 60 percent increase in the tactical nuclear forces deployed in western Europe.

A 75 percent increase in airlift capability.

A 100 percent increase in general ship construction and conversion.

A sixfold increase in counterinsurgency forces.

McNamara also pointed to the "demonstrated willingness to risk using these forces in defense of our vital interests. Here are some examples: The callup of about 150,000 reservists and the deployment of 40,000 additional men in Europe in the summer of 1961. The confrontation of Khrushchev on the issue of Soviet offensive missiles in Cuba in October of 1962. The dispatch of 16,000 U.S. military personnel to South Vietnam to assist that country with logistics and training support in combating the Vietcong insurrection."¹¹

As I have stated elsewhere, "The United States intended under Kennedy to develop a war fighting capability on all levels of violence from nuclear war to counterinsurgency."¹² The irony in analyzing the militarization of American foreign policy, and by inference, American life, can be found in a speech that President Kennedy was to deliver at the Trade Mart in Dallas on November 22, 1963. Dallas was one of the main industrial arteries of the national security state. Texas had gained much from the Cold War, and there was real concern that Kennedy was a secret dove. He intended to point out to the Texas audience that this was not the case. "In less than 3 years, we have increased by 50 percent the number of Polaris submarines . . . , increased by more than 75 percent our Minuteman purchase program, increased by 50 percent the portion of our strategic bombers on 15 minute alert and increased by 100 percent the total number of nuclear weapons available in our strategic alert forces . . . [We have] . . . radically improved the readiness of our conventional forces—increased by 45 percent the number of combat ready Army divisions, increased by 100 percent the procurement of modern Army weapons and equipment, increased by 100 percent our ship construction, conversion and modernization program, increased

¹¹ House Committee on Armed Services, *Sundry Legislation Affecting the Military Establishments*, Testimony of Robert McNamara, p. 6899 (1964).

¹² Marcus G. Raskin, "The Kennedy Hawks Assume Power from the Eisenhower Vultures," *Essays of a Citizen: From National Security State to Democracy* (Armonk, N.Y., 1991), 52–53.

by 100 percent our procurement of tactical aircraft, increased by 30 percent the number of tactical air squadrons . . . Finally, moving beyond the traditional roles of our military forces, we have achieved an increase of nearly 600 percent in our special forces" namely, those forces that were used in South Vietnam.¹³

The Kennedy policy on armaments and doctrine merely begins the puzzle. There is no question that President Kennedy intended to pull 1,000 advisers out of Vietnam by the end of 1963, and there is some evidence to support the view that he intended to pull all advisers out by 1965. According to Roger Hilsman, a former assistant secretary of state who dealt with Southeast Asia and held repeated discussions with President Kennedy, it was Kennedy's intention to work out a negotiated settlement along the lines of the one with Laos.¹⁴ Yet there is a wrinkle here. Ngo Dinh Nhu, who was assassinated at the same time as his brother President Ngo Dinh Diem on November 2, 1963, had sought to work out a settlement with the North Vietnamese and the Vietcong. This diplomatic intervention was interrupted by their assassinations. The Kennedy administration had, according to Henry Kissinger, a "direct role" in the coup against Diem "which led to his assassination."¹⁵ The Kennedy policies were ambiguous. On the one hand, the president had made numerous public statements to the effect that the war had to be won by the South Vietnamese government, not by the United States. On the other, the war was seen as something of a game of chess that the United States could walk away from any time it desired; but, if the cost was not too great, we should continue to play.

Theodore White, in his book *In Search of History*, claims that Kennedy intended to get out of Vietnam and not go forward with a full-scale war. According to Kenneth O'Donnell, Kennedy's principal political adviser, "Kennedy had just pledged to Senate Majority Leader Mike Mansfield not only the immediate withdrawal of one thousand troops of the sixteen thousand troops in Vietnam, but the withdrawal of all of them after the 1964 election." When O'Donnell asked Kennedy how he meant to do that, he quipped, "Easy, put a government in there that will ask us to leave."¹⁶ This part of Stone's contention appears to be on solid ground.

A QUESTION REMAINS ABOUT PRESIDENT JOHNSON and his interest in going to war in Indochina. Although Johnson had made a trip to Vietnam in 1961 and came back trumpeting the Walt Rostow-Maxwell Taylor thesis, there is also evidence that he too was dubious about war on the Asian mainland. In 1954, when Johnson was the majority leader of the Senate, he made clear to Eisenhower and John Foster Dulles that he would not support American military intervention at Dien Bien Phu, nor could he carry the Senate with such support unless Britain and

¹³ John F. Kennedy, undelivered speech, November 22, 1963. *Public Papers of the Presidents of the United States* . . . (Washington, D.C., 1980).

¹⁴ Roger Hilsman, Letter to the Editor, *New York Times* (January 20, 1992).

¹⁵ Henry Kissinger, *White House Years* (Boston, 1979), 231.

¹⁶ Theodore H. White, *In Search of History: A Personal Narrative* (New York, 1978), 531.

France fought side by side with the United States. So what caused his shift in point of view to supporting the Kennedy hawks' position?

As Stone intimates, there were a number of high-level national security meetings with Johnson immediately after Kennedy's death. The meeting that is best known was with leading advisers of the Kennedy administration, namely Dean Rusk, McGeorge Bundy, McNamara, Maxwell Taylor (Chair of the Joint Chiefs), John A. McCone of Central Intelligence, and Ambassador Henry Cabot Lodge. At that meeting, it was decided to go ahead with the war even though Taylor and McNamara had prepared at Kennedy's request a two-volume study, NSAM 263, which outlined a withdrawal program. This was presented at an NSC meeting on October 5, 1963. Nevertheless, Taylor, an early proponent of brush-fire wars, and McNamara, a convert to the same sublimated war doctrine, were hawks. In the fateful days after the Kennedy assassination, they told Johnson that the war was local and limited and that, with Diem out of the way, the American commitment should be strengthened with U.S. combat forces because the war could be won. Rusk had a strong commitment to American intervention in Indochina because of his belief in the Sino-Soviet bloc relationship (an alliance that had already ended) and because, as Rusk put it, the Soviets "blinked" during the Cuban missile crisis; thus they were not likely to be a factor in inhibiting American intervention. It was thought by one assistant secretary of state that Bundy, who knew better, went along for reasons of ambition. He hoped to replace Rusk in 1965 as secretary of state and then end the war. Taylor wanted to test out his flexible-response theories.

At this stage, we can only speculate on President Johnson's motives. One view is that his understanding of the Kennedy policy was to go forward with the war and that any softening on his part would have brought down the wrath of Robert Kennedy upon him. The attorney general had been a strong proponent during the Kennedy period of covert operations and sublimated war engagements. These included engagement in Vietnam, but Robert Kennedy ceased to be a friend of the CIA.

Johnson, who had imbibed the metaphors of Munich and falling dominoes, was reinforced by his own insecurity about foreign affairs and his fear that he would be blindsided by the Kennedy advisers, some of whom had wanted him off the ticket in 1964. He would be seen as an illegitimate imposter to the presidency who tampered with Kennedy's stated policies. (Given Robert Kennedy's stated positions, Johnson could not have imagined that Robert Kennedy would blindside him from a dove rather than a hawk position.)

The role of the military during this violent period is also ambiguous. The generals who had fought in the Korean War, MacArthur and Ridgeway, were adamantly opposed to war on the mainland of Asia. General Matthew Ridgeway was a charter member of the "never again" club. Having replaced MacArthur in the Korean War, Ridgeway argued in a continuous barrage of memoranda that the United States should steer clear of an Asian land war. However, the Taylor position held sway, and it led quickly to dramatic escalations.

It should be remembered that in the initial stages of the war it was the civilian hawks left over from the Kennedy administration who rallied around a land war.

They believed in the idea of a world-wide conflict with communism and in the domino effect; that is, if Vietnam fell, all of Southeast Asia was doomed. This was not Kennedy's view, Theodore Sorensen's view, or that of the few "doves" of the Kennedy administration who, after the Cuban missile crisis, believed strongly in accommodation. The policy of *détente* had been reflected earlier in Kennedy's successful settlement of the Laotian war, his calling off the Cold War, and his interest in pressing forward with a general and complete disarmament treaty. In a May 6, 1963, memorandum to the leading national security advisers, Kennedy voiced his deep concern about the arms race and ordered the government to prepare extensive plans for general and complete disarmament. It is likely that the president was reacting to Jerome Wiesner, the science adviser who in December 1962 had told and written Kennedy that the McNamara defense build-up was an unmitigated disaster for the national security of the United States, that it forced the Soviets to follow the United States in the arms race, thereby making the United States less secure.¹⁷ The Cuban missile crisis underscored his advice.

Would any of the top Kennedy advisers for reasons of ambition, a need to test out a pet theory, for money, sex, or because of ideological persuasion participate or initiate a coup? There seems to be no hard evidence for such a conclusion. Is it possible that, as with the death of Thomas Becket, a culture of violence and command would give some conspirators at a lower level inside and outside the government the idea that a murder should occur? This is somewhat more likely, given the conflating of crime and political intrigue in the covert and military world of that time.

The film points to two figures who are unnamed. One is the Donald Sutherland character, who is probably Colonel Fletcher Prouty. The other is his boss, who sends security specialist Prouty off to the South Pole to prevent him from ensuring Kennedy's safety in Dallas. His boss would have been General Edward Lansdale, who had cut his teeth in the 1950s by making Ramon Magsaysay a hero of Philippine limited land reform and the victor against the Huk rebellion. Lansdale then promoted Magsaysay into the presidency of the Philippines and went on to work on different schemes for pacifying Vietnam and Cuba. It does not seem likely that as hawkish as Lansdale or the first-level advisers were, they would have been involved in a plot to kill President Kennedy in order to press a war and prove their ideological doctrines. However, it should be noted that most of Kennedy's advisers were far more hawkish than he. For example, advisers such as Walt Rostow, who later succeeded Bundy, had urged the use of American ground troops and the bombing of the North. As I have noted, this refrain was also sung by Johnson when he was vice-president and by General Taylor, who, prior to becoming chairman of the Joint Chiefs of Staff, was Kennedy's personal military adviser. The willingness of these advisers to kill others does not mean that they intended to or did kill the president of the United States. There is no question, however, that a culture of violence was integral to that period and that it permeated all levels of government.

¹⁷ Jerome B. Wiesner to President John F. Kennedy, memorandum, December 4, 1962, John F. Kennedy Library, Waltham, Mass.

JFK also raises the question of CIA involvement in the assassination. It should be noted that those concerned with analysis of data and political trends in the Agency were eager for the United States to work out a negotiated settlement in Vietnam. To that end, they made public a document carrying the CIA's official imprimatur and calling for an end to the war in 1964. However, the question of CIA involvement in the assassination is much less easy to slough off when one notes the intimate connection the CIA had with Cuban exile groups in Miami. For that matter, the FBI also had connections with groups it had infiltrated or with individuals it used as informants as well as with extreme rightist businessmen it protected who hated President Kennedy. These connections occurred to the Warren Commission as well.

One recommendation the commission made was that the FBI, CIA, and other agencies should inform the Secret Service when a potential threat existed to the president's life. "Since these agencies are already obliged constantly to evaluate the activities of such groups, they should be responsible for advising the Secret Service if information develops indicating the existence of an assassination plot and for reporting such events as a change in leadership or dogma which indicate that the group may present a danger to the President. Detailed formal agreements embodying these arrangements should be worked out between the Secret Service and both of these agencies."¹⁸

It is hardly surprising that the culture of violence extending into the presidency through national security decisions also allowed government officials to use organized crime and, when necessary, to confront it. Robert Kennedy as attorney general initiated a "war on crime" at the same time that the CIA sought aid from criminals in attempting to assassinate Castro.

DISCUSSIONS GO ON ALL THE TIME about killing the president. These inchoate conspiracies abound in the nation. They come to very little. That they exist, however, should not be denied. That they are exacerbated by a culture of violence is obvious. And that the Warren Commission saw its primary goal as calming the people is clear. At the time, Bertrand Russell claimed that "there has never been a more subversive, a more conspiratorial, unpatriotic or endangering course for the security of the United States and the world than the attempt by the United States Government to hide the murderer of its recent President."¹⁹

The nation is again undertaking an Oedipal odyssey, looking for itself through this heinous murder. If we are mature enough to continue the search, it is well that not only the files of this terrible tragedy be made public but that the various people who have had access to these files be subject to oaths to assure the citizenry that documents have not been destroyed. Another step will have to be taken: files of the CIA, Department of Justice, and defense agencies will now have to be opened so that we may understand more completely the culture of violence that enveloped the nation during the Cold War period. We will not be able to assess a

¹⁸ *Report of the Warren Commission*, 440.

¹⁹ Bertrand Russell, quoted by Salisbury, *Report of the Warren Commission*, xxv.

single-bullet or lone-assassin theory unless we recognize the billion-bullet/nuclear weapons-and-missile system that dictated and framed our society's reality. Most important, however, if we choose to continue with national security state secrets of the kind that enveloped the assassination of Kennedy, then the nation psychologically will continue to be tortured by lies. The culture of violence and secrecy will hold sway; cynicism and alienation in its nastiest political sense will grow even greater.

AHR Forum
JFK: The Movie

MICHAEL ROGIN

THE SAN FRANCISCO CBS TELEVISION AFFILIATE KPIX sandwiched a news report between the 1992 Super Bowl and the "60 Minutes" interview with Governor Bill Clinton and Hilary Rodham Clinton about the Democratic presidential candidate's alleged marital infidelities. Featuring a Hollywood movie, *JFK*, the putative news story reported demands, driven by the film's notoriety, to open the secret files of the House Select Committee on Assassinations. The real news, however, was the movie itself. Television news mixed newsreel videotape of the controversy surrounding the film with shots from the movie, which was, as television viewers could see, a jumble of fictional and documentary footage. *JFK* stands at the confluence of two developments, one old and one new, that came together with the election of a Hollywood actor, Ronald Reagan, as president of the United States: the conflation of politics and conspiracy, and the confusion between politics and the fiction-making visual media.

Although *JFK* has been the target of unprecedented hostile attention from journalists and political commentators, four books making Kennedy the victim of one conspiracy or another made the *New York Times* best-seller lists the first week in February; the number one non-fiction best seller was by Jim Garrison, the former New Orleans district attorney who is the hero of Oliver Stone's movie.¹ That, in addition, the journal of the American Historical Association should publish a symposium on a Hollywood movie, with contributions written within weeks of the film's release (what about research and time for reflection?) is itself source material for the future historian of late twentieth-century America. As professionals attack *JFK*'s conspiracy theory, millions of Americans rush to see the film and buy the books, and the gulf between the political class and the apparently pre-political mass public could not be wider.

"Treason doth never prosper, what's the reason? For if it prosper, none dare call it treason." These lines, quoted by Stone's Garrison, call Richard Hofstadter from his grave, for they are featured in his classic book, *The Paranoid Style in American Politics* (1965). The couplet forms the epigraph for *None Dare Call It Treason*, John Stormer's John Birch Society exposé, published the year after

I am indebted to conversations about *JFK* with Cathy Gallagher, Richard Hutson, and Kathleen Moran.

¹ See Stephen E. Ambrose, "Writers on the Grassy Knoll: A Reader's Guide," *New York Times Book Review* (February 2, 1992): 1, 23–25.

Kennedy's death, of the communist takeover of Washington.² Stone's conspiracy is anti-communist. As *JFK* unfolds, it reveals that an omnipresent "they" killed not only John Kennedy but also Robert Kennedy and Martin Luther King, Jr., that "they" seized power in a "coup d'état," and that Lyndon Johnson was an "accomplice after the fact." Stone's assassins murdered Kennedy to stop him from withdrawing from Vietnam, making peace with Cuba, and ending the Cold War. But "they" killed a president who (as the movie does not say) increased military spending, heated up Cold War rhetoric, intensified the American intervention in Vietnam, and sponsored, until his own assassination, death plots against Fidel Castro.

Resembling traditional American conspiracy theories, Stone's demonology makes an easy target for those defending the allegedly beleaguered political elites smeared by *JFK*. From their perspective, the syndicated political commentator William Pfaff's, for example, Oliver Stone is a New Left McCarthyite. But such a view is maliciously ahistorical. Kennedy was no New Left hero, for either civil rights activists in the early 1960s (since his Justice Department and FBI worked against them) or for the anti-war movement that emerged after his death. Stone, in turn, is a product not of the rise of the New Left but of its demise. Blaming the New Left counterposes *JFK*'s paranoia to a rational governing class, making it impossible to understand either the power of the movie or where it goes wrong. Stone's films assault the viewer, and some commentators have protected themselves by keeping their distance from *JFK*. But if we accept the invitation to enter the Kennedy assassination from Garrison's point of view (I refer throughout to the film's Garrison, Kevin Costner), then we can trace the path from legitimate political disorientation to the moment when Garrison reaches obsession.

Since the publication of Edward Epstein's *Inquest* a quarter-century ago, reasonable people have had to doubt the Warren Commission, lone assassin, "magic bullet" (as Garrison calls it) version of the killing of Kennedy.³ (Stone's defense of his movie, in a February 3, 1992, letter to the *New York Times*, focuses entirely on the deficiencies of the Warren Commission.) In the first portion of *JFK*, a disorienting montage draws the viewer into the evidence of other assassins, other bullets, other places from which Kennedy may have been shot. Rarely have the camera shot and the gun shot been more aligned, with the viewer at once behind the telephoto lens and, like Kennedy, its target.

Reasonable people have also had to acknowledge, for a quarter-century, the power of secret government in the United States, hidden both in its unaccountable decision making at the top and its covert operations on the ground. The achievements of that government (many of which are, by the same technique of discontinuous assault, detailed in the film) include: the recruitment of Nazis to work for the CIA in the Cold War; the CIA-sponsored coups against Jacobo Arbenz in Guatemala and Muhammad Musaddiq in Iran; the FBI, Military

² Richard Hofstadter, *The Paranoid Style in American Politics* (New York, 1965), 110–11; John A. Stormer, *None Dare Call It Treason* (Florissant, Mo., 1964).

³ Edward Jay Epstein, *Inquest: The Warren Commission and the Establishment of Truth*, Richard H. Rovere, intro. (New York, 1966).

Intelligence, and CIA operations against domestic dissent; Watergate; Iran-Contra.

It is plausible, moreover, to link the Kennedy assassination to secret government interventions during the Cold War. Since the Watergate burglars were anti-Castro Cubans implicated in Kennedy's plots to kill Castro, President Nixon justified the Watergate cover-up on national security grounds, to keep secret the Kennedy-Cuba connection.⁴ Nixon imagined Lee Harvey Oswald as Castro's avenger. Leftist versions of the assassination propose other ties: to anti-Castro Cuban exiles, to the Cuban exile/Mafia/Kennedy tangle; to people in the national security bureaucracy; to the family of deposed South Vietnamese President Ngo Dinh Diem, murdered in the Kennedy-sponsored coup. The scenarios bewilder by their number and believability. Evidence for the withholding within government of information that might shed light on Kennedy's death is overwhelming. To attend seriously to Cold War politics and the Kennedy assassination is to risk being thrown back into the paranoid position (to use psychoanalyst Melanie Klein's term) of helpless, suspicious disorientation.⁵

The widespread feeling that America began to fall apart after Kennedy was killed prolongs national mourning; conversely, the extraordinary fixation on *JFK* is evidence of the public malaise. But the unresolved assassination, combined with Kennedy's complicity with the forces suspected of doing him in, has blocked a national mourning of the president as he actually was, encouraging the regression from what Klein calls the depressive position, where loss can be acknowledged and overcome, to idealization, splitting, and paranoia.

A plausible version of the assassination, like Don DeLillo's *Libra* (1988), makes sense of the chaos surrounding Kennedy's death, but the price of sanity-restoring, narrative coherence is that the story be presented as fiction. *JFK* refuses the fictional label by insisting it has discovered the truth. But that rejection of fictional narrative entails another, of form as well as content, for Stone replaces a convincing, novelistic, plot-as-story with a mysterious, fragmentary, plot-as-conspiracy.

The elements of a plot in both those senses are set in New Orleans. Stone, however, provides no characters whose actions connect his sordid New Orleans revelations to the Washington scene of the crime. In the exception that explains the rule, a Washington messenger turns one of Garrison's staffers into a tool of the cover-up, preparing for a scene that will discredit the Mafia assassination theory (which contaminates Kennedy) by putting it into the renegade's mouth.

Stone has no problem finding anti-communist Kennedy haters, among both Bay of Pigs veterans and home-grown right-wingers. He accepts their view of Kennedy, inverts it, and makes it the instrument of the president's death. But to give the assassination its cosmic political significance, as the coup d'état source of all that has gone wrong in the country, Stone (himself a Vietnam veteran) also needs a group that feared Kennedy was withdrawing from Southeast Asia. It is

⁴ See Fawn M. Brodie, *Richard Nixon: The Shaping of His Character* (Cambridge, Mass., 1981), 493–96.

⁵ Melanie Klein, *Love, Guilt, and Reparation and Other Works*, R. E. Money-Kyrle, intro. (New York, 1975).

harder to give verisimilitude to that story. As a result, *JFK*'s political content and filmic method come to mirror the conspiracy the movie is supposedly exposing. When narrative history fails Stone, his plot splits in two: idealization of the beautiful "dying king" on the one hand, demonization of a homosexual band on the other. Sexual anxiety overwhelms politics, in *JFK*'s paranoid style, as a homosexual primal horde slays the young father-king.

Although Garrison complains that the government infantilizes its citizens by keeping them from the truth, his Americans are never adults; they are Hamlets, "children of the slain father-leader whose killers still possess the throne." Stormer's *None Dare Call It Treason*'s dedication—"to Holly, May her future be as bright as mine was at age 5"—speaks equally to the cry of betrayed innocence that drives *JFK*; Stone's film is "Dedicated to the young." The beautiful object of the viewer's desire in the nostalgic newsreel footage we watch along with Garrison, Kennedy is felled by the perverted desire of David Ferrie and Clay Shaw. Stone's Kennedy is at once the "father-leader" whose killing unleashes chaos and the beautiful young man (synecdochical for Garrison and the male viewer) endangered by erotic attraction.

With David Ferrie (the pilot linked to the CIA and Operation Mongoose), homophobia and conspiracy each first enter the movie, joined together on Ferrie's body. An announced "alleged homosexual incident," preceding the report of his anti-Castro activities, frames our first view of Ferrie. His flimsy story supposedly makes Garrison suspect a plot, but what fills the screen is Ferrie's nervous, flitty manner. That a middle-aged degenerate drove to Dallas with "a couple of young friends" only to hunt birds raises sexual as much as political suspicion. The two come together again in the figure of the attractive, corrupted, imprisoned homosexual prostitute, Willie O'Keefe. (Unlike Ferrie and Shaw, this figure, played by Kevin Bacon, is Stone's invention.) In the conspiratorial connections with which O'Keefe floods Garrison, sexual and political perversions are entirely intertwined. Disguise is at the heart of the "homosexual underground," O'Keefe tells the district attorney. "You don't know shit because you've never been fucked in the ass." Graphic words and images depicting some men dominating others, rather than a political narrative, links invisible Washington power to New Orleans sex. The male prostitute propositions Garrison when the interview is over. The district attorney's interrogation and trial of the homosexual businessman, Clay Shaw ("the guy's a fag"), now organizes Stone's conspiracy. Homosexual blackmail, perverted sexual practices, and murder merge in Ferrie's confession to Garrison, shortly before he is murdered in turn. Kennedy was killed, the film comes close to saying, because he refused to submit to homosexual domination.

When Garrison's wife accuses him of caring more about Kennedy than his own family, she points to the absence of heterosexual desire that feeds the homosexual threat. (From one side, *JFK* inherits Stone's misogyny; from the other, it derives from *No Way Out*, the 1987 espionage thriller in which the character played by Kevin Costner is framed for murder by a homosexual in love with the real killer, his State Department boss.) Homosexual panic may not be the universal ground of paranoia, as Freud argued in the Daniel Paul Schreber case, but it organizes *JFK*. Schreber believed that invisible rays emanating from an authoritative source

were turning him into a woman and forcing him to give birth. Such an omnipotent force slays the president; first in the shooting, then in the horrifying reenactment of Kennedy's autopsy, the extraction of his brain from his head, its instruments violate, for Stone as for Schreber, the vulnerable male body.⁶

Sensory overload characterizes Stone's film technique in general, but whereas flashbacks and editing establish eyewitness authority for the conspiracy, the homosexual scenes carry the weight of emotional disturbance. Montages of body parts, a transvestite bacchanal, and the strange movements of Ferrie and Shaw overwhelm visual and narrative coherence. Just as Stone blends the camera shot and the gun shot, so his rapid cutting, sudden close-ups, and bodily dismemberments join the filmic to the sexual fetish. Cinematic form enforces the disorienting fragmentation; homophobia is its content. Fragmentary details pregnant with meaning are the building blocks both of the content of political demonology and of Stone's paranoid film style. The director employs montage to return to the primitive, pre-illusionistic beginnings of motion pictures. Unlike classic narrative films, his images disperse rather than tell a story. But, unlike primitive cinema, Stone puts spectacle in the service of narrative. Intentionality at the top organizes the charged data of Stone's animistic universe. Conspiracy supplies the formal and final causes (in Aristotle's classification) that restore psycho-political order.

Whereas the fragments are disturbingly visible, however, the unity can only be told. The Abraham Zapruder film of Kennedy's assassination is shown over and over, frame by frame, as if it held the key to the plot, but the visual bludgeoning leads to confusion, not unambiguous conclusion. Only words keep Zapruder from turning into *Blow-Up*, Michelangelo Antonioni's 1969 film in which murder remains mysterious because the picture keeps the secret of whether it has a secret at all. Coherence in *JFK* is supplied by the longest monologues in Hollywood history. These voice-overs, spoken from within the diegesis and illustrated by streams of juxtaposed images, offer the structure for which the viewer, even more than before entering the theater, now longs. But the monologues cannot make actual connections, any more than could a traditional filmic narrative, for those would be vulnerable to exposure as fiction. *JFK*'s Deep Throat authorizes the conspiracy, his soliloquy supported by flashbacks that are keyed to his subjective account but shown as historical truth. Urging Garrison to bring Shaw to trial without knowing how everything fits together, this paternal figure prepares us to experience Shaw's acquittal as evidence that the conspiracy goes on. Garrison's thirty-five minute speech to Shaw's jury brings the film to an end.

Demonology imagines that a secret power is exercised on the body; thus sexual fantasy has always been part of the American paranoid style. In Maria Monk's antebellum, anti-Catholic, non-fiction best seller, for example, priests kidnap and engage in criminal intercourse with nuns.⁷ Women's liberation, interfering with

⁶ Daniel Paul Schreber, *Memoirs of My Nervous Illness*, Ida Macalpine and Richard H. Hunter, trans. and ed.; Samuel Weber, intro. (Cambridge, Mass., 1988); Sigmund Freud, "Psycho-Analytic Notes on an Autobiographical Account of a Case of Paranoia (Dementia Paranoides)," in *Standard Edition of the Complete Psychological Works of Sigmund Freud*, 24 vols., James Strachey, ed., vol. 12 (London, 1953-74).

⁷ Maria Monk, *Awful Disclosure of the Hotel Dieu Nunnery of Montreal* (1836; rev. with appendix, New York, 1977).

the rape-and-rescue erotics of female victimization, may contribute to the shift from heterosexual to homosexual sadomasochism; if this postfeminist movie does not target threatening women, it marginalizes them instead. But the wish that women go away returns to haunt male connections. Moreover, although the story presents homosexual contagion as the cause of the assassination, the spectacle presents it as the consequence, since we meet the primal horde knowing that Kennedy is dead. Homosexual contagion is at once source and result of the killing, making the spread of alternative sexualities one more disaster for which Kennedy's death is to blame.

But the sexual politics of *JFK* is perhaps more the product of Washington men than feminists and gays. It illustrates with particular, sensate force how disorienting powerlessness invades the psyche, threatening to turn men into receptacles for sadomasochistic possession. (American male impotence as the tragedy of Vietnam is explicit in Stone's *Born on the Fourth of July* [1989] and almost as close to the surface in George Bush's Vietnam syndrome.) *JFK* deserves the attention it is getting neither as a political understanding of the assassination and its aftermath nor as a McCarthyite assault on vulnerable elites but rather for making us experience how politically produced paranoid anxieties, somatized on the visually produced mass body, turn into paranoid analysis.

AHR Forum
JFK: Historical Fact/Historical Film

ROBERT A. ROSENSTONE

TO THOSE OF US INTERESTED IN HISTORICAL FILMS, the fuss in the media over *JFK* feels familiar. Complaints that the film bends and twists history; accusations that director Oliver Stone willfully mixes fact and fiction, fails to delineate clearly between evidence and speculation, creates characters who never existed and incidents that never occurred—these are the sorts of charges made every time a historical film on a sensitive subject appears. With *JFK*, the controversy is particularly heated because of both the topic and its treatment. The film hits us with a double whammy: one of America's most popular directors not only explores our recent history's most touchy subject but does so in a bravura motion picture that (maybe it's a triple whammy) also takes a highly critical stance toward major branches of the American government.

Complaints over the misuse of history in film seem to be based on two notions: first, that a historical film is no more than a piece of written history transferred to the screen and thus subject to the same rules of historical practice; and, second, that a fact is a fact and history is little more than an organized compilation of such facts. We who write history should find these assertions questionable. At the least, we have to be aware that "facts" never stand alone but are always called forth (or constituted) by the work in which they then become embedded. In order to evaluate the way in which any work of history—including the motion picture—uses facts (or data) to evoke the past, we must investigate the aims, forms, and possibilities of the kind of historical project in which those data appear.

All this is to say something simple but important: a film is not a book. To judge the contribution of a work like *JFK*, we must try to understand just what it is a historical film can do.

As a dramatic motion picture, *JFK* comes to us in a form that has been virtually unexplored by people interested in the study of past events. Neither historians nor filmmakers have given much thought to the most basic questions about the possibilities and standards of history when it is represented in the visual media. Evaluations of historical films in essays and reviews are always made on an individual basis. Certainly, the historical profession has no agreed-upon way to answer any of the following questions: What kind of historical knowledge or understanding can a historical film provide? How can we situate it in relation to written history? What are its responsibilities to the historical "fact"? What can it tell us about the past that the written word cannot?

Such questions are too broad to answer here, but they are good to keep in mind as we think about *JFK*. My aim in what follows is less to deal with the contributions and shortcomings of the film than to approach it as part of a tradition. I want to situate *JFK* as both a certain kind of film and a certain kind of historical film. Placed in this context, the factual “errors” (if one wants to term them that) of the work will appear to be less the fault of the filmmaker than a condition of the medium and the kind of movie he has chosen to make. The contributions (if one wants to call them that) of the film, on the other hand, are in large measure its own. They derive less from the form of the film than from the way that form has been put to use.

THERE IS NO SINGLE WAY TO DO HISTORY ON FILM. The traditional division into the dramatic work and the documentary is increasingly irrelevant as recent films (*JFK* included) often blur the distinction between the two. My own research has suggested that history on film comes in a number of different forms. *JFK*, despite the many documentary elements it contains, belongs to what is certainly the most popular type of film, the Hollywood—or mainstream—drama. This sort of film is marked, as cinema scholars have shown, by a number of characteristics, the chief being its desire to make us believe that what we see in the theater is true. To this end, the mainstream film utilizes a specific film language, a self-effacing, seamless language of shot, editing, and sound designed to make the screen seem no more than a window onto unmediated “reality.”

Along with “realism,” four other elements are crucial to understanding the mainstream historical film:

Hollywood history is delivered in a story with beginning, middle, end—a story with a moral message and one usually embodied in a progressive view of history.

This story is closed, completed, and, ultimately, simple. Alternative versions of the past are not shown; the *Rashomon* approach is never used in such works.

History is a story of individuals—usually, heroic individuals who do unusual things for the good of others, if not all humankind (ultimately, the audience).

Historical issues are personalized, emotionalized, and dramatized—for film appeals to our feelings as a way of adding to our knowledge or affecting our beliefs.

Such elements go a long way toward explaining the shape of *JFK*. The story is not that of President Kennedy but of Jim Garrison, the heroic, embattled, incorruptible investigator who wishes to make sense of Kennedy’s assassination and its apparent cover-up, not just for himself but for his country and its traditions—that is, for the audience, for us. More than almost any other historical film, this one swamps us with information. Some of it, in the black-and-white flashbacks that illustrate the stages of the investigation, is tentative or contradictory. (So much is thrown at us that, on a single viewing, the viewer has difficulty absorbing all the details of events discussed and shown.) Yet, even if contradictions do exist, the main line of the story is closed and completed, and the moral message is clear: the assassination was the result of a conspiracy that involved

agencies and officials of the U.S. government, the aim of the assassination was to get rid of a president who wished to curb the military and end the Cold War, and the “fascist” groups responsible for the assassination and the subsequent cover-up are a clear and continuing threat to what little is left of American democracy.

LET ME PUT IT SIMPLY: if the conventions of the mainstream historical film make it difficult for such works to create a past that stays within the norms by which we judge written history, certain other factors make it impossible. It is not just that most of the data by which we know the past comes from the realm of words and that the filmmaker is always involved in a good deal of translation from one medium to another, attempting to find a visual equivalent for written evidence. It is also that the mainstream historical film is shot through with fiction or invention from smallest of details to largest events. (Historians do not, of course, approve of fiction, aside from the underlying fiction that the past itself can be truly told in neat, linear stories.) Invention occurs for at least two reasons: the requirements of dramatic structure and the need of camera to fill out the specifics of historical scenes.

Drama demands the invention of incidents and characters because historical events rarely occur with the kind of shape, order, and intensity that will keep an audience in its seats. Inventions move the story forward, keep emotions high, and order complex series of events into plausible structures that will fit within filmic time constraints. When *JFK* creates a fascist, homosexual prisoner named Willie O’Keefe to give Garrison the evidence that Clay Shaw was involved with Lee Harvey Oswald, or invents a Deep Throat character in Washington (played by Donald Sutherland) to help Garrison make sense of all the evidence he has gathered by providing a theory to hold it all together, one can see that Oliver Stone is doing no more than finding a plausible, dramatic way of summarizing evidence that comes from too many sources to depict on the screen.

Invention due to the demands of the camera may be a subtler factor, but it is no less significant in shaping the historical film. Consider, for example, something as simple as the furnishings in a room where a historical character sits—Jim Garrison’s office or conference room, or Clay Shaw’s apartment. Or think of the clothing that characters wear. Or the words they speak. All such elements have to be approximate rather than literal representations. They say: this is more or less the way Garrison’s room looked in 1966, or these are the kinds of clothes a character might well have worn, or these are likely examples of the words he or she spoke.

The same is true of individuals. This is not just a matter of the director making up characters. Even historical people become largely fictional on the screen. The very use of an actor to portray someone is itself a fiction. If the person is an actual historical figure such as Garrison, even if the actor looks like the figure (which is not true in *JFK*, for actor Kevin Costner looks little like the real Garrison, who in turn does not look much like Justice Earl Warren, the character he portrays), the film on a literal level says what cannot truly be said: not just that this is how the

person looked but also that this is how he moved, and walked, and gestured, and this is how he sounded when he spoke.

To analyze a historical film is to see how small fictions—settings and clothing, the look and sound of characters—shade into larger and larger inventions. Even the tiniest sorts of fictions are not unimportant factors. At least, not if history is about the meaning of past events. In a medium in which visual evidence is crucial to understanding, such pervasive fictions are major contributors to the meaning of the film, including its historical meaning. So, too, is that elusive, extra-historical element, the aura carried by famous actors and actresses. A star like Kevin Costner, fresh from his award-winning *Dances with Wolves*, cannot simply disappear into the character of Garrison. From that film, he carries for many in the audience a strong feeling of the decent, simple, honest American, the war hero who more than a century ago was critical of a certain kind of expansionist militarism in American life.

LIKE A HISTORY BOOK, a historical film—despite Hollywood's desire for "realism"—is not a window onto the past but a construction of a past; like a history book, a film handles evidence from that past within a certain framework of possibilities and a tradition of practice. For neither the writer of history nor the director of a film is historical literalism a possibility. No matter how literal-minded a director might be, film cannot do more than point to the events of the past. At best, film can approximate historic moments, the things that were once said and done, but it cannot replicate them. Like the book, film will use evidence to create historical works, but this evidence will always be a highly reduced or concentrated sample; given its limited screen time, the film will never provide more than a fraction of the (traditional) data of a scholarly article on the same topic. Even as a lengthy, three-hour film that includes an unusually dense barrage of information, *JFK* must often make major points with sparse evidence or invented images. Within the world of the film, the idea that Kennedy was ready to withdraw American troops from Vietnam, for example, rests on the mention of a single memorandum and the testimony of a fictional character. The notion that black Americans loved Kennedy is conveyed by having a single woman say, "He did so much for this country, for colored people."

What I am suggesting is this: the Hollywood historical film will always include images that are at once invented and yet may still be considered true; true in that they symbolize, condense, or summarize larger amounts of data; true in that they carry out the overall meaning of the past that can be verified, documented, or reasonably argued. But, one may ask, how do we know what can be verified, documented, or reasonably argued? How do we know whether Kennedy was about to withdraw troops or whether he was loved by African Americans? Both of these highly debatable points must be answered from outside the film, from the ongoing discourse of history. From the existing body of historical texts. From their data and arguments. This need for outside verification is not unique to film. Any work about the past, be it a piece of written, visual, or oral history, enters a

body of preexisting knowledge and debate. To be considered “historical,” rather than simply a costume drama that uses the past as an exotic setting for romance and adventure, a film must engage the issues, ideas, data, and arguments of that discourse. Whatever else it does or does not do, *JFK* certainly meets these requirements as a work of history.

The practice of written history is not a single kind of practice. And if that practice is dependent on data, its value and contribution have never been wholly a matter of those data and their accuracy. Certainly, different works of history use data in different ways, make different sorts of contributions to our understanding. Some works of history may be important chiefly for the data they create and deliver, others for their evocation of people and events of a vanished time and place. Some historical works are noted for their elegance of argument or skill at representation, others for raising new questions about the past or for raising old questions for a new generation.

It is the same with historical films. They come in different forms and they undertake different historical tasks. Some evoke the past, bringing it to life, giving us an intense feel for people, places, and moments long past—this surely is one of the glories of the motion picture. (Who can sit through *JFK* without reliving many of the agonies of the 1950s and 1960s that it depicts?) But film may do more than evoke: the historical film can be a stimulus to thought, an intervention into history, a way of re-visioning the past. We do not go to the Hollywood historical film for data but for drama. For the way it intensifies the issues of the past. For the way it shows us the world as process, makes us participate in the confusion, multiplicities, and complexities of events long past.

JFK is a film that undertakes more than one historical burden. Because it chooses as its central strategy an investigation of the past, the film has a self-reflexive edge, one that suggests much about the difficulty of any historical undertaking and the near impossibility of arriving at definitive historical truths. More important, perhaps, *JFK* makes an apparently old issue come to life—indeed, the reaction it has evoked makes it seem like a very successful piece of historical work. Not a work that tells us the truth about the past but one that questions the official truths about the past so provocatively that we are forced once again to look to history and consider what these events mean to us today. Like a good historian, Stone begins *JFK* with a preface that contains a thesis; he uses President Dwight Eisenhower’s farewell address, with its warning about the possible effect of the military-industrial complex on the future of our country, to set the stage for a film that will illustrate the prescience of Ike’s words. By doing this, Stone forces us to face the kind of large issue that a more sober historian, mired in a slough of information and worried about the judgments of professional colleagues, might find difficult to raise so sharply: has something gone wrong with America since the early sixties?

Director Oliver Stone has been faulted for thinking that many changes in the United States stem from a single act, the killing of John Kennedy, but others who are less sanguine about the judgments and actions of Kennedy may take him as a symbol. Certainly, the experience of the film, like that of any important work of history, resonates well beyond the ideas of its creator and speaks to and for those

who do not share Stone's strong faith in Kennedy. When assessing *JFK*, one should ask this question: who else in America has dared to raise such historical issues so powerfully (or at all) in a popular medium? If it is part of the burden of the historical work to make us rethink how we got to where we are, and to make us question values that we and our leaders and our nation live by, then, whatever its flaws, *JFK* has to be among the most important works of American history ever to appear on the screen.

Reviews of Books

GENERAL

ORLANDO PATTERSON. *Freedom*. Volume 1, *Freedom in the Making of Western Culture*. New York: Basic Books. 1991. Pp. xviii, 487. \$29.95.

DONALD W. TREADGOLD. *Freedom: A History*. New York: New York University Press. 1990. Pp. viii, 459. Cloth \$40.00, paper \$20.00.

Reading a book on freedom in the West is often like revisiting a well-loved house. The furniture is the same, perhaps a bit rearranged, a new acquisition here or there, an old standby relegated to the attic. Still, one is consciously at home.

The almost simultaneous publication of two scholarly essays on freedom reminds us how long this sense of place has taken to develop, how essential it is in the artificial structure known as Western culture, and how uncongenial and disorienting it has been, and is, to some within and to nearly all outside the household of the West.

Donald W. Treadgold's book has the wider chronological span, from pre-history to 1990. For him, freedom is equivalent with political pluralism, incorporated above all in democratic forms of government and social orders that safeguard the independence of the individual in person and property. Freedom is optimistic; it is "everlasting" in the human spirit. His numerous and distinguished studies of Russia and China during the last two centuries anticipated the later chapters in this book. He describes how ideals and institutions of freedom, which continuously developed from the beginnings of Western civilization through the Enlightenment, expanded with quite checkered and precarious results into Latin America after 1492 and, also with entirely distinct careers, during the Age of Colonialism, into India, China, and Japan. In their diffusion, they left no traces on other parts of Asia "east of Turkey and Israel" (p. 370).

Transmutations of the state system, which began their onslaught suddenly between completion and publication of this book, have rendered some conclusions more provisional than the author could have foreseen, certainly those concerning Yugoslavia and the former Soviet Union. Possibly events will have cast a strange light over other conclusions before the

publication of this review. The rare breadth of learning and the intellectual boldness of Treadgold's book are demonstrated most strikingly in the concluding chapters, where the author surveys with great concision the entire histories of India, China, Japan, and Latin America (the last chiefly since Columbus). The earlier chapters provide an incremental account of the formation of ideals and ideologies of freedom in three great sources of Western civilization (Old Testamental Judaism, Athens, and Rome). Treadgold goes on to chronicle their assimilation to the pluralistic institutions of medieval Europe, refinement (notably by means of doctrines on the freedom of conscience) in the Renaissance and Reformation, and vindication under the challenge of absolutism, with the resulting establishment of constitutional, democratic forms of government.

As a genre, the encyclopedic history of freedom is poetic, and the life mimicked by its art is normally that of the author. Thus, Hegel, Engels, John Stuart Mill, John Bagnell Bury, and John Dewey all refracted quite different ideals of freedom through the prism of personal experience and commitment. Possibly because Treadgold's autobiographical traces in the book have recognizable precedents in the tradition of writings about liberty in Western culture, his inventory of the house of freedom is also familiar. He was able to omit Africa and to relegate slavery to an incidental role.

The life mirrored by Orlando Patterson's art is a different, and rather less familiar, kind. Africa is present, pretextually, in the original inspiration that this book received from studies of the African slave trade and the antislavery movement and, textually, in the normative role assigned to slavery throughout the entire history of freedom in this account. It is present subtextually in the sociological coordinates with which Patterson frames the study and contextually in his earlier, celebrated, investigations of slavery.

For Patterson, slavery is the precondition of freedom. In fact, freedom and slavery become conceivable and intelligible dialectically, through one another, much as do good and evil. His interpretation is a variation on the older dialectic of liberty and authority, which characterized authority as tyranny of magistrates, religion, or dominant class interests and

opinions. Patterson's substitution of slavery for authority in this dialectic came about through the experience and reflection that led him to conclude that "at its best, the valorization of personal liberty is the noblest achievement of Western civilization" (p. 402), but one that, from the beginning of European civilization, was imprinted with "Europe's most loathsome heritage of racism" (p. 357). Patterson's interpretation took shape in a gap, unknown to Treadgold, between what he calls "the Western past" and what he calls "my past" (p. xiii). The method by which Patterson unfolds the stages of this contaminated and contaminating heritage combines history with sociology.

Patterson ends his account in this first volume with the fourteenth century, reserving "the modern history of freedom" to "a later, shorter work" (p. xvi). The expansion of ideologies of freedom outside the boundaries of Europe has not yet entered his narrative, but, considering that the valorization of freedom was complete by the fourteenth century, he has had occasion to explain, from the beginning, why the universal institution of slavery did not engender a universal dialectic with freedom. It was essential, he maintains, that personal freedom be valued and endowed with the capacity of self-determination, and that it have prerogatives and worth beyond those demanding obedience to the group, class, or corporation. Such personal freedom must be cherished and defended by a critical mass, an articulate and powerful subclass of society. It must confer rights to participate in the conduct of political affairs. Thus were combined what Patterson calls the three notes in "the uniquely Western chord of freedom": personal (freedom to act as one wills and can), "sovereign" (freedom to act as one wills, reserved in autocracies to the ruler alone), and civil (freedom to participate in governance of a centralized political order). In different degrees, these converged (with their dialectical opposite, slavery or serfdom) in classical Athens, the Roman empire, and, with increasing completeness, medieval Europe, but not elsewhere.

Patterson holds that slavery belonged to an artificial, or "unnatural," social and political order that men imposed on nature. Coercive power was the basis of that order. As a social value, personal freedom defied the artificial hierarchy of compulsion and had to be intruded on by agents other than dominant male elites. In his consistent effort to recover the voices of the historically voiceless, Patterson concludes that this was first done by women in classical Athens, as portrayed by tragic dramatists, appealing beyond the changeable limits of law to the universal order of natural justice and, heedless of penalty, enacting the unwritten obligations to self and kindred even in defiance of human law. Their empathy with slaves enabled Athenian women to be "the first creators of Western freedom" by shifting the valorization of freedom from social order to nature. Throughout later periods, in other societies, women

continually reconstructed "a distinctively feminine version of the value" after men had assimilated and modified the "note" of personal freedom in prevailing social ideologies (p. 398).

Much of Patterson's subsequent argument depends on his hypotheses regarding the dialectic of slavery and freedom. Are these foundation stones securely laid? One is aware from the outset that Patterson's subject is morphology rather than phenomena. Diverse, and diversely gathered, data are collated to identify "culture-characters" that transcend differences "in time, place, and levels of sociocultural development" (p. 15). Thus, the materials with which he frames his conceptions come from the ancient Near East (including the enslavement of Hebrews in Egypt), from recent studies of pre-Columbian Cherokees, and from anthropological research in Africa, the South Seas, Polynesia, and Brazil.

Morphology has its own freedom from detail, but there is reason to wonder whether artistic license of brilliant, darting eclecticism tends to deny evidence its own voice. Inevitably, examining Greek tragedians through Patterson's lens produces some novel interpretations of classical texts. Euripides provides the bulk of his proof. Euripides has long been known as not typical but eccentric, the one Athenian tragedian who gave prominence to the sufferings of women, the exceptional writer who (in Edith Hamilton's judgment) was daring enough to set "a poor ignorant peasant beside a royal princess and show him at least her equal in nobility" (*The Great Age of Greek Literature* [1942]). Before Hamilton, others regarded Euripides as "the first modern mind" because of his compassion for those who suffered the consequences of male ideals. She, and they, also acknowledged Euripides as an "archheretic" writing against the prevailing values of his time. Patterson's assertions that women drew empathetic associations of themselves with slaves are occasionally hypothetical reconstitutions rather than readings of ancient texts. Moreover, it would be difficult to imagine that a reader of this book heard a woman's voice in a late-twentieth-century man's interpretation of dramas written more than two millennia ago by men to be performed by men before audiences of men.

There is much to admire in Patterson's boldness, in his wide and comprehensive knowledge, in his regard for analytical distinctions, and in his ability to draw vast and disparate materials into an intelligible and intriguing exposition. As the study moves from antiquity on, however, the dominant morphology does not allow one willingly to suspend disbelief. Where is the evidence that conceptions of freedom in Christianity were framed without reference to theodicy, and that this indifference to human understanding of how there could be evil if God were omnipotent and omniscient (and thus perfectly good) "goes back to Paul" (p. 326), when, in fact, so much reflection about the freedom of the will began with the Apostle Paul's queries on why the Jews were first chosen and then

reprobate, why God elected the foolish things of the world to confound the wise, why He had mercy on some but sent "strong delusion" to others "that they should believe a lie," and why the righteous suffered tribulation?

It is no small matter to omit the theodical component from a system of thought preoccupied with suffering and cursed human nature as made in the image and likeness of God, particularly regarding intellectual and volitional freedom. Patterson thereby draws a great divide between his morphology and the patristic and medieval materials to which he refers. This omission is, however, of a piece with the discount that he places on the connection between freedom and wisdom and on the regard for the pursuit of wisdom and education, as a process of emancipation that was cherished from antiquity onward. The values that he imposes on patristic texts are not the ones that their authors deposited in them.

According to Patterson, the culminating moment in the formation of the ideology of freedom came in the Middle Ages. European civilization was complete; freedom was its lodestar. Many specialists would argue that generalizations about liberty become increasingly difficult to draw from the twelfth century on. With the development of professional classes, meanings proliferated. Regional practices imposed quite distinctive modifications on teachings inherited from ancient and patristic cultures. As earlier, Patterson gives precedence to unprivileged social orders that were excluded from what used to be called the great humanist tradition.

Although he refers to learned authors, he rests his case largely on his own interpretation of serfdom, burgeoning into new hybrids of slavery, and on the status of degraded and oppressed members of society other than serfs, including heretics. While this emphasis assists the development of his overarching interpretation, it does distract him from a more easily established fulcrum of emancipation. In medieval texts the word "liberty" commonly denotes property—the "liberty" to collect a toll, to build a bridge, to hang a man. To receive a "liberty" established obligations, notably toward its grantor. In such instances, exercising freedom entailed discharging obligations. Frequently, the sign and guarantee of a liberty was a charter or contract. Without expecting this volume to violate its chronological limits by providing a discussion of the social contract's place in the history of freedom, one does rather miss an account of its origins.

The coordinates of the received scholarly tradition concerning the range of medieval speculation have hardly varied from the account given by M. C. D'Arcy in "Authority and Freedom in Medieval Europe" (in *Man's Right to Knowledge*, ed. J. B. Brebnew [1954], 45–55). Certainly there is room for zealous, even iconoclastic, analysis to supplement those coordinates or place them in a wider constellation.

Patterson's inventory of the house of freedom

includes more unfamiliar entries than Treadgold's. There is reason to ask that some of the more surprising ones be verified. There is no question, however, that Patterson's lens has also refracted more obviously than Treadgold's the sufferings that the ideology of freedom has brought with it through war, persecution, enslavement, and oppression. This, too, is a familiar theme (although not in the variations conferred on it by Patterson), arising in the satires of Voltaire and passing through many stages to Franz Fanon's arraignment of humanism as the antithesis of humanity, and now formally lodged in historiography. It inspires some with hatred for the grandeur and beneficence canonized in Western ideals of freedom; from others, it calls forth improvisations on theodicy, in a human key.

KARL F. MORRISON
Rutgers University,
New Brunswick

ALAN WATSON. *Slave Law in the Americas*. Athens: University of Georgia Press. 1989. Pp. xv, 179. \$25.00.

Alan Watson's book on the slave laws of the Americas fills an important gap in the historiography of slavery. The author seeks to understand the differences between the law in English America and in Latin America. To this end, he focuses on the ease or difficulty of manumission, the degree to which slaves were accorded legal personality, their treatment in criminal law, and the limitations on the masters' ability to mistreat their slaves. Watson's central thesis is that "the legal culture and legal traditions themselves can provide a sufficient explanation for the main features of a legal institution" (p. xiv).

To develop his thesis Watson discusses the slave laws of ancient Rome, as well as those of the Spanish, English, French, Portuguese, and Dutch slave societies of the Americas. He finds that, with the exception of the English, the slave law of the European colonies drew its inspiration from the laws of ancient Rome. Roman law had been received in Spain, France, Portugal, and the United Provinces and their slave law in the Americas reflected this tradition. This meant, among other things, that manumission was obtained rather easily, the masters' private power of discipline was limited, and freed slaves found a place in society.

The English colonies had a different legal trajectory. England did not receive Roman law so their colonial slave law had no legal precedent. Since there was also no legal authorization for slavery in the colonies, the statutes evolved in accordance with local needs, coming "into being bit by bit either by statute or by judicial precedent, sometimes based on what people did" (p. 64). Accordingly, English slave law reflected local realities to a far greater extent than that of the other European colonies.

Watson maintains that Roman slave law constitutes "the paradigm case for nonracist slavery" (p. 76). Slaveholding was seen as a matter between the owners and the owned and, consequently, the law lacked a public dimension. In contrast, English America developed racist slavery and the statutes reflected the need to regulate the relationship between the black slaves and the larger white society. Watson notes that "in English America one might almost say that a slave belonged to every citizen—at least he was subordinate to every white" (p. 66). The other societies of the Americas also developed racist slavery but, according to Watson, the European tradition on which their slave law was based ensured that it remained "nonracist in its rules" (p. 133).

Watson clearly underestimates the racism that permeated the law of all colonial societies. The differences that existed between English law and the others was a matter of degree, not of kind. Although he is sensitive to the impact of social, cultural, and economic forces on the shaping of slave law, Watson chooses not to pursue this area of enquiry. Had he done so, he would have enhanced the importance of this work for students of slavery. Watson's conclusion that the relative ease with which masters freed their slaves in Spanish, Portuguese, French, and Dutch America compared with English America "was the result of differences in the legal tradition in Europe rather than societal conditions in the Americas" (p. xii) is provocative but not adequately substantiated in this study. Nevertheless, this slim volume deserves the serious attention of all students of comparative law and the law of slavery.

COLIN PALMER
University of North Carolina,
Chapel Hill

ANTHONY KEMP. *The Estrangement of the Past: A Study in the Origins of Modern Historical Consciousness*. New York: Oxford University Press. 1991. Pp. viii, 228. \$32.50.

Anthony Kemp's well-written meditation on historiography has an ambitious reach in the range of authors it seeks to represent, and in its claim to offer an innovative approach to the theory of history. Carefully chronological, it begins with texts by Africanus, Eusebius, and Augustine and then makes its way through medieval historiography to the Protestant Reformation. The thesis turns on the interpretation of John Wycliffe, Martin Luther, and John Foxe, who are viewed as revolutionary in their insistence on the difference between their own era and all that has preceded them in Christian history. American Puritan historians are then analyzed as the primary developers of this "supersession" model of history (p. 110), concluding with a two-page discussion of Jonathan Edwards. An epilogue adds Thomas

Prince and Ralph Waldo Emerson, ending with Henry Adams as the epitome of contemporary consciousness: "history has not gone beyond Adams; his historical world is ours" (p. 174).

The purported scope of the book is reduced considerably when one realizes that the works selected do not represent the history of history in the Christian tradition so much as they represent the favorite historians of New England Puritanism. "Supersession" as a theoretical model merely emphasizes the Puritans' own belief in the importance of being last. Counterposing this model to the early Christian belief in the unity of Christian history yields the supposed revolution in historical consciousness that is the focus of the book. Unfortunately, this is simply a restatement of what has been said already about the history of typological thought.

Although Kemp writes more clearly than many literary critics, there is a loss of complexity in his reiteration. The book is useful in its cursory characterizations of works, particularly if the reader has not had the opportunity to read the complete texts in question. Some of these histories are long and others are difficult to obtain. Students will nevertheless find it useful as an introductory guide to major historical texts of the English and American Protestant tradition. But there is no engagement with the theory of major critics within the field such as Sacvan Bercovitch, and no attempt to relate this book to poststructuralist debates about modern consciousness. The complex tension between the discourse analysis of literary theorists and the recent work of Foucauldian historians is not addressed. Kemp dodges this issue, as well as the whole area of material culture, by simply declaring at the outset that history is really literature, that "the past has no perceivable existence beyond its literary expression" (p. vii). Readers might be persuaded to travel this familiar route once more if it involved new turns made possible by recent developments in narrative theory among literary critics, but Kemp only refers us back to Hayden White. Although Kemp notes a number of major theorists such as Michel Foucault and mentions them in passing, his book seems isolated from the main issues in contemporary theory—an isolation that seems all the more strange in a book about the structure of modern consciousness.

ANN KIBBEY
University of Colorado,
Boulder

DAVID LEWIS SCHAEFER. *The Political Philosophy of Montaigne*. Ithaca: Cornell University Press. 1990. Pp. xv, 407. Cloth \$41.50, paper \$13.95.

In an interpretation that is clearly indebted to Leo Strauss and his followers, David Lewis Schaefer seeks to describe Montaigne's *Essays* as fundamentally a work of political philosophy. Schaefer defines politi-

cal philosophy as the overarching science that gives coherence to all human endeavors, and he maintains that Montaigne's specific purpose was to lay the theoretical and moral foundations for what would become modern liberalism. The book is thoroughly researched—displaying a command of the major literary as well as philosophical scholarship—well written, and densely argued.

Schaefer begins by disengaging himself from recent scholarship, most notably that concerning the evolution of Montaigne's thought. Although Schaefer admits that there are changes in style and emphasis between the three major editions of the *Essays*, he nonetheless maintains that these changes were "a thoroughly planned rhetorical technique adopted by Montaigne so as to have a maximal influence on his readers at minimal danger to himself" (p. 27). In other words, the *Essays*—from their inception and in their entirety—are a wholly self-conscious work of political philosophy, intentionally veiled by a rambling and superficially contradictory style.

In a detailed, three-chapter analysis of the "Apology for Raymond Sebond"—the central portion of the *Essays*—Schaefer lays the foundation for his interpretation. He argues that Montaigne rejected both Christianity and traditional philosophy because they had distracted human attention from earthly woes, and that Montaigne pointed the way toward a new form of practical philosophy whose goal was the progressive amelioration of the human condition. In his analysis of the rest of the text, Schaefer elaborates on the dimensions and repercussions of this practical philosophy: that it was fundamentally reformative rather than conservative, republican rather than monarchical, creatural rather than spiritual, and that it ultimately served to establish the doctrine of individualism underlying modern liberalism. Such a brief review cannot do justice to the (sometimes maddening) intricacy of Schaefer's analysis.

It is difficult for a historian to evaluate a work of Straussian political philosophy, especially given the ahistorical nature of this school of thought. Suffice it to say that Schaefer's work exhibits the characteristic strengths and weaknesses of this school. On the positive side, he engages in a close reading of the text, subjecting certain essays to a virtual line-by-line analysis. On the negative side, this reading is less subtle than single-minded, relentlessly reducing even the most recalcitrant material to a single body of "teachings," largely by positing a deeper reading of the text that overrides surface inconsistencies. Historians may well be reluctant to accept that Montaigne—who wrote over a twenty-year period, dramatically expanding the *Essays* with each major edition—was from the start a political reformer with a preconceived agenda, one so cleverly hidden from the censors that it must be painstakingly decoded. For those disinclined to accept this premise, Schaefer's

argument—despite its earnest intensity—fails to convince.

ZACHARY S. SCHIFFMAN
Northeastern Illinois University

STEPHEN K. SANDERSON. *Social Evolutionism: A Critical History*. (Studies in Social Discontinuity.) Cambridge, Mass.: Basil Blackwell. 1990. Pp. xviii, 251.

Historians interested in theories of social evolution will find Stephen K. Sanderson's book a spirited, fair-minded, and useful survey and critique of these works, written variously by sociologists, anthropologists, archaeologists, and biologists, from the time of Herbert Spencer, Lewis Henry Morgan, and Edward Burnett Tylor to the present. After flourishing in the late nineteenth century, Sanderson explains, social evolutionism came under attack by Franz Boas and his disciples in the early twentieth century, underwent a partial revival between 1930 and 1960 in the writings of V. Gordon Childe, Leslie White, and Julian Steward, surged ahead in the 1960s and 1970s under the leadership of Robert Carneiro, Gerhard Lenski, and Marvin Harris, gaining new adherents among biologists such as Edward O. Wilson, only to suffer new attacks by Maurice Mandelbaum, Robert Nisbet, Anthony Giddens, and others. Sensing the tide of opinion turning against social evolutionism, Sanderson sets out "to expose the considerable mythology that exists in regard to social evolutionism and in consequence to show that certain versions of evolutionary theory are intellectually defensible" (p. 223). Some types of evolutionism should be abandoned, says Sanderson, others purged of dubious elements, "but world history can still be—indeed, should be—understood in evolutionary terms" (p. 223).

A social evolutionist, writes Sanderson, is "someone who is attempting to identify and explain general directional trends in history" (p. 205). Unlike the historical sociologist, who tends to focus on particular historical situations, the social evolutionist compares historical developments in different places and times, seeking to discover and explain parallel developments and thereby to develop a general theory of social change. Among such theories Sanderson rejects as unscientific all "developmentalist" theories that view human history as the unfolding of some master pattern or idea; evolutionary formulations must, "like Darwin's account of biological evolution," explain historical change by ordinary causes (p. 3).

Among such ordinary causes Sanderson prefers demographic, ecological, technological, and economic causes (with "a certain amount of 'superstructural feedback'") as against "idealist" factors such as symbolic codes, legal norms, religious or philosophical systems, "or some other phenomenon that is primarily mental or ideational" (p. 113). He is not sympathetic, however, to the attempt of sociobiolo-

gists like Wilson to reduce historical causation to genetically based "epigenetic rules." Although attracted by the idea of the unity of science and by the analogies between the concepts of evolutionary biology and those of social evolutionism, Sanderson concludes that the agendas of biologists and sociologists are only partially overlapping and that social evolutionism will continue, as in the past, to develop largely independently of evolutionary biology.

What is the work-a-day historian, generally not deeply interested in high-flown theories of history, whether philosophical or "scientific," to make of Sanderson's ambitious critique? Without question one will come away from it with a good grasp of major contributions to social evolutionary theory and of the arguments for and against each of them, illustrated in many cases with specific historical examples. But this book, as Sanderson notes, is basically analytical and critical rather than historical—"a *critical* history, not a *critical history*" (p. 7). Thus, despite his preference for "materialist" as against "idealist" interpretations of history, Sanderson makes no attempt to explain the development of theories of social evolution in terms of "material" factors. Instead, he treats it as a purely intellectual enterprise, with only an occasional suggestion of underlying ideological biases. In this respect his book differs widely from that of his hero, Marvin Harris's *The Rise of Anthropological Theory* (1968).

Both writers, however, believe devoutly in the possibility of a science of historical evolution analogous to, but not conceptually isomorphic with, the science of evolutionary biology and capable ultimately of explaining scientifically such major transformations in human history as the Neolithic Revolution, the rise of civilization and the state, and the transition from feudalism to capitalism. Obviously, these are topics of great interest to historians, who should welcome whatever help may be available from their social scientific colleagues.

JOHN C. GREENE
University of Connecticut,
Storrs

FELIX GILBERT. *History: Politics or Culture? Reflections on Ranke and Burckhardt*. Princeton: Princeton University Press. 1990. Pp. ix, 109. \$14.95.

These essays, written over the past ten years, were published only a few months before Felix Gilbert's death in early 1991. Gilbert was the last surviving student of Friedrich Meinecke, who in turn represented a tradition going back in its political and cultural conceptions to Leopold von Ranke and to the Prusso-German school of historians. Meinecke had broadened the conception of history of this tradition by linking politics and ideas. At the same time Meinecke was among the very few historians who supported the Weimar Republic and was one of the first

to warn of the Nazis. Tolerant of a diversity of views, he attracted the most innovative young scholars of the time—among them Eckart Kehr, Hajo Holborn, Hans Rosenberg, Ernst Simon, Dietrich Gerhard, and Gerhard Masur, all of whom went into exile after January 1933. Gilbert's works reflect the broad perspective of the Meinecke circle. His writings, which ranged from the Renaissance to the twentieth century, combined an interest in intellectual, cultural, political, and diplomatic history with a concern for the questions that informed the theory and practice of historians from Machiavelli and Francesco Guicciardini until the present.

It is from this background that Gilbert reads Ranke and Burckhardt. In his eyes Ranke's great contribution was to link a rigorous commitment to critical methods with a concern for what he understood to be universal history, which in fact was only that of the European world. At the same time Ranke endowed historical prose with the qualities of great literature. Burckhardt appears to Gilbert as the antithesis of Ranke who nevertheless in many ways accepted and developed Rankean ideas, the vision of a transnational European world, and the conception of the historian as at once scholar and artist. Burckhardt had been trained in Ranke's seminars and, even in his later years when he had gone in very different ways, acknowledged his debt to his teacher. He admired especially Ranke's history of the papacy and the history of the Reformation in Germany because of their universalism, the link they established between politics and culture, and their literary qualities. Ranke's histories of France and England, written later, represented a decline, with their focus on a nation. Burckhardt shared Ranke's conservatism but not his confidence in the solidity and morality of the modern state. Instead Burckhardt embraced a stark pessimism regarding the state in an age of nationalist violence and cultural vulgarity.

Gilbert does not share the political and cultural elitism inherent in Ranke and Burckhardt's view of history and hence also eschews their conservatism. His writings on historiography reflect his openness to the new ideas and interests of his time. Yet in essential ways he carries on the heritage of Ranke and Burckhardt without losing sight of its limitations.

GEORG G. IGGERS
State University of New York,
Buffalo

RICHARD WOLIN. *The Politics of Being: The Political Thought of Martin Heidegger*. New York: Columbia University Press. 1990. Pp. xvii, 221. \$29.50.

Since the books on Martin Heidegger by Victor Farias (*Heidegger et le nazisme* [1987]) and Hugo Ott (*Martin Heidegger. Unterwegs zu seiner Biographie* [1988]) there is no longer any doubt about the extent of his involvement with National Socialism. It was neither

temporary nor, as Heidegger later claimed, a mere *pro forma* matter. When the famous philosopher joined the Nazi Party and when he assumed the rectorship of the Albert-Ludwig University of Freiburg, he made a public statement of a distinctly political, if not philosophical, nature. The question left to be resolved is the exact relationship between Heidegger's philosophy and his political views and activities.

Naturally, scholars have addressed themselves to this problem; at one extreme Jürgen Habermas insisted on the "internal connection" between Heidegger's thought and politics, and at the other extreme Richard Rorty cautioned us against attempts to relate the two areas, and Sidney Hook dismissed such attempts as "parochial."

Of all the disciplines intellectual history is one of the most treacherous: at every step representation risks becoming misrepresentation. Richard Wolin has courageously faced his subject with a systematic analysis of Heidegger's "Politics of Being," and has approached this task with impressive learning and ingenuity, even though the end result may be something of a tour de force. He places the "axial relation" between fundamental ontology, as elaborated by Heidegger in his great work *Sein und Zeit* (1927), and political engagement with Nazism in the context of Nietzsche's *Kulturkritik* and its admittedly distorted transmission by the German so-called "conservative revolutionaries." Moeller van den Bruck, Oswald Spengler, Carl Schmitt, and, most important, Ernst Jünger. No doubt Heidegger's work must be read as a statement on the crisis of the European mind—in his own words, the "atrophy of authentic life," conditioned by the relentless advance of secularization, technology, and urbanization. No doubt Heidegger capitalized on the cultural pessimism that has gripped the German mind since the turn of the century and encouraged German mandarins to stake out their preserve, namely a vision of a German way (*Sonderweg*) defined by *Kultur* (as against *Zivilisation*) and *Gemeinschaft* (as against *Gesellschaft*). By thus proceeding to pose the "question of Being" that would lead to a recovery of the lost authenticity, the century's greatest philosopher (at least in the Continental tradition) came to strike a "devil's bargain," as Wolin rightly puts it, with the most barbarous regime the world has ever known (p. 15). The infamous and much discussed *Rektoratsrede* of May 1933 was not, then, merely a great blunder, as Heidegger later called it, but an integral chapter of a *Weltanschauung* whose logic led to naked decisionism, a prescription of danger, daring, and death, and a refusal to come to terms with the inevitability of the flawed reality of a modern world. In his political thought, Heidegger thus sacrificed the plurality and difference of human practical life, in Wolin's terms, "on the altar of an atavistic Eleatic totem—the totem of 'Being'" (p. 14).

The inquiry into Heidegger's "Politics of Being" is unquestionably legitimate and necessary; moreover,

especially in the case of Heidegger, the man of fame and distinction, the issue of responsibility and indeed guilt has to be raised. His later bravado statement that "who thinks greatly must err greatly" only adds to his record of errors. But once all this is said, it must be pointed out that the Heidegger case transcends the dimension of a German *Sonderweg* or *Sonderweg*-awareness. In this respect, even as superb an intellectual historian as Wolin may have succumbed to the hazards of misrepresentation. Was it merely a fluke that after World War II Heidegger became the philosopher of the French *Résistance*? How can we explain the "philosophical actuality" of Heidegger in the non-European countries like contemporary Japan and China, and the fact that he has become the patron saint of the *filosofia de la liberación* in Latin America? What can we make of the fact that the politics of Václav Havel, the Czechoslovak resister-turned-president, were distinctly an instance of the "Politics of Being"? Not only the German *Sonderweg* but also these avenues must be explored to do full justice to the bedeviling case of Heidegger. Despite or because of his errors he remains one of the truly important and inquisitive philosophers of our age. The questions he asked about the discontents of our civilization and especially about the place of technology in our world are central to our existence.

Altogether this is a splendid book: vigorously argued but at the same time cautious, provocative but at the same time thoroughly responsible and discriminating.

KLEMENS VON KLEMPERER
Smith College

JOSEPH A. AMATO. *Victims and Values: A History of a Theory of Suffering*. Assisted by DAVID MONGE. Foreword by EUGEN WEBER. Paperback edition. New York: Praeger. 1990. Pp. xxvi, 236.

Joseph A. Amato's history of suffering is an intellectual historian's answer to literary critic Elaine Scarry's work, *The Body in Pain: The Making and Unmaking of the World* (1985). Whereas Scarry was impressionistic and daring, Amato is careful and focused. Amato seeks to document two basic problems: first, how have thinkers—mainly theologians and philosophers—explained suffering? And second, has the nature or quality of suffering changed over the passage of history and from culture to culture? Amato carefully compiles theories of suffering and sorts them historically. This is an area into which Scarry's ahistorical approach did not enter. Amato's focus is the West, which presents him with limitations that he takes as universals. His is a Christian history of suffering. The structure of his book is that of the early twentieth-century cultural historian. The first "real" chapter in his history begins with the Greeks (with a bow to "anthropological" sources such as Sir James Frazer) and their complex attitudes toward pain and suffer-

ing. He follows this with chapters on Christianity. Here he subsumes the Jewish attitudes toward pain and sacrifice to the role of Christian precursors. (On this question he should have read Theodor Reik or Mary Douglas on the meaning of Jewish ritual practices as independent of Christian tradition.)

Amato's presuppositions about the centrality of Christianity in the definition of pain and suffering in the West preclude his seeking other resources for the history of suffering. What is most surprising in the survey is that Amato assumes that the theories of pain and suffering directly reflect the perceptions and feelings of those who suffered. What would have been a natural resource for him—from the Greeks up to the present day—is the complex medical literature on pain and suffering, from the discussion of wounds in Hippocrates to the post-traumatic stress disorder of late-twentieth-century psychiatry. In this book we have a history of ideas that may or may not reflect "realities." On the contrary, the theories may be in part attempts to answer the received theories about pain, to present counterarguments about the perception of pain as documented by those (often mechanistic) thinkers such as physicians who directly deal with the "body in pain."

Much less successful than Amato's survey of the theories of suffering is his attempt to understand the catastrophes of suffering, specifically his discussion of Shoah. Amato is by training and interest a historian of modern Christianity. (He is the author of a book-length study of Emmanuel Mounier.) Thus, he casts his understanding of Shoah in terms of Christian martyrology: "No kingdom has been as morally powerful in post-World War II America as the Holocaust" (p. 181). Amato takes the Christian perspective that Shoah was a martyrdom of many peoples, including the Jews, and was one of a long history of such martyrdoms. Amato sees Shoah as "consciously chosen to be the Jews' Crucifixion" (p. 181). This is very much in line with his lineal reading of Western history as a means of dealing—through religious and secular patterns, but always in some metaphysical or transcendental manner—with the meaning of suffering. Here I would argue that Amato is (unconsciously) presenting the Christian reading of David Roskies's brilliant and self-consciously aware study of Jews and their own sense of suffering. It is within the debate about the meaning of suffering that it becomes clear that two very different theories of suffering—that of Judaism and that of the Christianity—come into conflict.

Amato's work appears to be a general history of suffering. In reality it is a very limited reading of certain Christian and secularized Christian views of this problem. He attempts to reinterpret as parochial the Jewish reaction to symbols of suffering, such as to the opening of a convent on the grounds of the death camp at Auschwitz. He believes his own approach is a "neutral" one based on his own development of a

theory of suffering. In fact, it is a clear, Christian reading of the meaning of suffering. It is obvious from this volume that this is a problem in the history of perception that demands an understanding of the necessity of multiple, often contradictory representations of identical experiences.

SANDER L. GILMAN
Cornell University

PHILIP D. CURTIN. *The Rise and Fall of the Plantation Complex: Essays in Atlantic History*. (Studies in Comparative World History.) New York: Cambridge University Press. 1990. Pp. xi, 222. Cloth \$39.50, paper \$10.95.

In this concise volume, Philip D. Curtin, long a leading scholar of the Atlantic economy, offers a series of thematically linked essays intended to place the plantation complex in the perspective of world history. Curtin identifies the plantation complex as a distinctive form of cultural encounter between Europe and non-Western societies and seeks to treat it as an entity. Although he conceives of the plantation complex broadly to include not just economic but also political, social, demographic, and cultural aspects, he consciously excludes its North American segment from consideration in this work. In his view, the plantations of the American South differ from those of the Caribbean in their demographic, social, and economic structure as well as in the timing and pattern of their historical development. The Caribbean plantations, Curtin asserts, more adequately express the character of the mature plantation complex. Furthermore, by concentrating on those developments that are less familiar to North American readers, he intends to put the plantation South in a broader historical and geographical context.

The organizing thread of the book is an account of the historical development of the plantation complex from its medieval origins to the abolition of slavery in the Americas during the nineteenth century. Curtin emphasizes the economic and political dimensions of the complex as a whole rather than the structure and operation of the plantations themselves. As the focus of the first part of the book shifts from the Mediterranean to the Atlantic islands, Brazil, and finally to the Caribbean, the essays trace the expansion and evolution of the plantation complex and delineate its role in linking Europe, Africa, and the Americas together to form an Atlantic economy. The second part of the book examines the political and economic reasons for the end of slavery in the Americas and reflects on the consequences of and responses to the decline of the plantation complex.

The development of the plantation is treated within a comparative framework that emphasizes the complexity of historical processes, including the role of disease and geography in shaping plantation regimes and patterns of colonizing enterprise. Curtin

pays particular attention to the sources of plantation labor and the forms of its organization. There are excellent chapters devoted to the African slave trade and its impact on Africa, and to the changing demand for labor in the Americas. These chapters not only examine the problem of labor but indicate as well the diversity and extent of trade implicated in the plantation complex. They are complemented by an equally fine treatment of the abolition of the slave trade and slave emancipation from a world historical perspective.

Although Curtin regards the sugar colonies of the eighteenth-century Caribbean as the apogee of the plantation complex, his discussions of the Mediterranean slave trade, slavery in Africa, and the use of Chinese and East Indian laborers after emancipation both extend the narrative and highlight the distinctive character of New World slavery and the transatlantic slave trade. In addition, Curtin's comparative historical approach leads him to examine forms of colonial expansion and settlement as well as slavery and other systems of forced labor outside the plantation zone. At times the treatment of these substantive topics is uneven, but these chapters usefully serve to place the evolution of the plantation in the broader historical context of European expansion.

Specialists may of course differ with particular interpretations or emphases in a work of this type. Nevertheless, this synthesis of the historical trajectory of the plantation complex within a comparative framework stands virtually alone. It is the product of decades of not only research and scholarly reflection but also of active teaching and experimenting with new themes and materials in the classroom. Although the author properly insists that this work is not a comprehensive textbook, it provides an excellent introduction for undergraduate students. In addition, it will offer new insights to experienced scholars and give them cause to reflect on familiar themes in a fresh light.

DALE TOMICH
State University of New York,
Binghamton

ALEXANDRA PARMA COOK and NOBLE DAVID COOK.
Good Faith and Truthful Ignorance: A Case of Transatlantic Bigamy. Durham, N.C.: Duke University Press.
1991. Pp. xvi, 206. \$21.95.

At last historians are able to read a true love story without pangs of guilt because of all the other, more "serious" books awaiting our time and thought. Alexandra Parma Cook and Noble David Cook have written that love story, and more. Their book is, as its subtitle summarily indicates, a case of bigamy that bridges the Atlantic. It is the story of a youth from a respectable *hidalgo* (lower nobility) family, Francisco Noguero de Ulloa, forced by a willful, widowed mother into an arranged marriage to the daughter of

a prosperous merchant, a woman and situation he apparently found so disagreeable that he eventually sailed for the Indies in 1534 (allegedly without consummating the marriage). There his life and adventures seem typical of the portrait painted of a whole generation of first conquerors by James Lockhart (*Men of Cajamarca* [1972]). He served the king, was rewarded with Indians, settled down to an honorable life as a leading citizen of an important provincial town, and, after learning of his Spanish wife's death, eventually married the well-educated, beautiful, and wealthy widow of one of the first Supreme Court judges of Peru. It is his second wife's insistence that they return to Spain that immediately embroiled him in a lengthy court case over bigamy. At worst, his sisters, two highly respected nuns, had lied in their letter about the death of his first wife; to be more charitable, they had written too soon, for his first wife did not indeed die from her malady, she recovered and pressed charges against her spouse on learning of his remarriage.

But this book is more than an entertaining reprieve. Noguero's story would have us add personal reasons (to escape a woman he in every respect seemed to detest) to the motivations for the conquest: gold, God, and glory. In addition, we are treated to the details of both arranged marriages. The first took place for obvious advantages of marrying money in return for lower nobility (and tax-exempt) status. The second marriage, if the feeling one gets reading between the lines is correct, was out of respect and admiration that soon turned to devotion and love, besides similar and favorable economic situations. In this respect, Noguero lived out the collective aspirations of many of the early conquerors by marrying well and retiring to Spain. The story goes beyond Ida Altman's book, *Emigrants and Society* (1989), because we see Noguero's life on both sides of the Atlantic. Personal connections in Peru and America were helpful to find employment and favor at court, as well as for gossip and news. The legal case itself shows the often slow course of litigation, jurisdictional disputes between church and state (à la J. H. Parry, *The Audiencia of New Galicia in the Sixteenth Century* [1948]), and the influence of money and powerful people over the outcome. All of this and more is detailed against the backdrop of the frontier atmosphere of civil war, rebellion, and factional fighting in Peru and the rise of Spain to the status of first-rate world power and center of an extensive and rich empire.

To its great credit, the book is written in a fresh, brisk, and moving style that probably would at least partially soothe Simon Schama's lament that late-twentieth-century historians have moved away from the "grand narrative" ("Clio has a Problem," *New York Times Magazine* [Sept. 8, 1991], 34).

SUSAN E. RAMIREZ
De Paul University

LEO SPITZER. *Lives in Between: Assimilation and Marginality in Austria, Brazil, West Africa, 1780–1945*. (Studies in Comparative World History.) New York: Cambridge University Press. 1990. Pp. xii, 250. Cloth \$39.50, paper \$11.95.

Occasionally a book with an unlikely subject will turn out to be fascinating, cut through topical barriers, and probe into regions seldom explored. Leo Spitzer has written such a work. At first glance, his family histories from Austria, Brazil, and Sierra Leone would seem unrelated. The progenitors were modest people born into an ostracized class (Jewry) or into slavery. Later generations clawed their way to respectability, only to find impenetrable obstacles to further mobility. Yet they lived in markedly different worlds, and their trajectories never crossed. Who would imagine that a unified story could emerge from these disparate lives?

Spitzer boldly addresses universal processes—the assimilation of subordinate peoples into dominant societies and the plight of the individuals forced to adapt—that exist everywhere humans congregate in any number. The full range of such experiences could not be captured with a survey, a biography, a single-country study, or an institutional approach, because the most dramatic interactions of peoples and cultures in the modern era spanned continents and took generations to work out. Nevertheless, where many would have concluded that the story, while important, could not be researched, Spitzer plunges into a multicultural and multidisciplinary study that not only succeeds but also offers inspiration to others of us who chafe at ordinary methodological limits. This is truly cutting-edge work.

The three remarkable families Spitzer follows eventually spawned prominent figures, people who mattered in recorded history: Stefan Zweig, André Rebouças, and Cornelius May. The first became a best-selling author in Europe, the second a leading engineer and abolitionist in Brazil, and the third mayor of Freetown, Sierra Leone. For a time these men lived in the limelight, apparently able to surmount prejudices against their people (Jews and negroes). They became beacons of hope.

The false liberalism of the late nineteenth and early twentieth centuries encouraged talented and creative members of subordinate (today called minority) groups to believe that the old barriers were falling. Religious and racial bigotry seemed to be vices of the past. Modern people surely judged others on their intrinsic merits and qualities. Then, tragically, the reaction set in. These three men—admirable figures in their own right—were ground into the dust by the terrible forces of racism. Zweig committed suicide while in exile from Nazi-controlled Austria, Rebouças probably committed suicide while in exile in Africa, and May perished in prison, broken in spirit and health by allegations of corruption. Their individual

catastrophes were intensified by the heights they and their families had scaled.

This book possesses a haunting quality that adds to its appeal. The author's preface explains that his family, related to the Zweigs, itself experienced the diasporic forces that drive the story. Spitzer turned up enough biographical and archival material to illuminate the lives of the later generations of Zweigs, Rebouças, and Mays, creating something of a collective biography. The reader tends to identify with these families and hope that their liberal aspirations will be fulfilled, only to see them quashed by the forces of racism, bigotry, and prejudice. Remarkably, the differences between European Jews, colonial Africans, and Afro-Brazilians are not so great that we cannot see these groups undergoing similar experiences. Ultimately the book succeeds like good biography, inducing the reader to see the world from others' points of view and thus to share in the human story.

In sections interspersed throughout the text, Spitzer discusses the sociological literature of assimilation and the psychological bases of marginality. Although critical to the analysis, these social science passages detract from the story and are mined with jargon. Otherwise, the writing is clear and straightforward.

This study entailed research in the United States, Europe, Africa, and Brazil, demanding the languages of those places and the patience needed in Third World settings. Gaps in the material are bridged with judicious use of historical imagination. Spitzer deserves our admiration not only for conceiving such a broad, multifaceted history but also for actually carrying it out. By breaking away from the usual forms, this book challenges all of us to be more comparative and universal in our quest for the human condition.

MICHAEL L. CONNIFF
Auburn University

CAROLE SHAMMAS. *The Pre-Industrial Consumer in England and America*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xi, 319. \$65.00.

Not too long ago most historians would have thought the phrase "pre-industrial consumer," the key concept in this important book by Carole Shammas, an oxymoron. Before the Industrial Revolution, the argument ran, the great bulk of the population either lived at a rudimentary level or achieved an adequate if modest self-sufficiency. Given such crude material conditions it would make little sense to study the consumer behavior of any but the most privileged inhabitants of the pre-industrial world. Such notions, and along with them the entire concept of an Industrial Revolution that neatly dichotomizes the history of the West, are now in rapid retreat in the face of a new understanding of the household economy of ordinary people in early modern Europe and its

American colonies. The early modern West, we have recently learned, witnessed substantial changes in diet as new commodities (especially tobacco, sugar, and tea) produced in the colonies became items of mass consumption, and in living patterns as small, inexpensive semidurables (clothing, buttons, cutlery, pottery, and so forth) produced in rural industries were widely distributed. The wealthy, of course, consumed more than their share, but "being poor and being a consumer were not mutually exclusive conditions" (p. 1) and the great bulk of the population participated in what has been called Britain's "empire of goods."

In this book, Shammas summarizes and extends this new scholarship. Part 1 explores demand for consumer goods, detailing the extent to which households were able to supply their wants through internal production and suggesting that it was as much shifting preferences as a failure of self-sufficient strategies or a process of proletarianization that drove increasing numbers toward the market. Part 2 describes the standard of living to show how shifts in diet, housing, and the ownership of durables changed the lives of ordinary people. Shammas shows that expanding long-distance trade networks and substantial price reductions made a range of new commodities available, and she explores ways in which interest in such goods led to shifts in preferences, in the allocation of work within households, and in social relationships among family members. Part 3 addresses the distribution system to suggest how the articulation of an extensive and efficient retailing network gradually undermined the hierarchical structures that had severely restricted consumer sovereignty by giving greater numbers of people direct access to things they wanted. Retailing has seldom been attended to by early modern economic historians. Shammas makes an important contribution by clearly establishing its centrality, identifying the major issues, and effectively summarizing recent scholarship while introducing some fresh research of her own.

Shammas pursues all these topics in a comparative framework that serves her purposes effectively. England and America, she notes, "shared the same trading network and many of the same cultural values, but their material situation . . . differed substantially" (p. 3), providing her the opportunity to test a variety of explanations for changing consumer behavior. And the topics are pursued with an inventive use of seemingly intractable sources (especially but not exclusively post-mortem inventories) and a sure-handed mastery of statistical techniques and economic theory. The result is a stunning achievement, an important book that forces us to look at familiar evidence in new ways, challenges widely held views of the pre-industrial world, and establishes the ordinary consumer as a central figure in the early modern economy.

RUSSELL R. MENARD
University of Minnesota,
Minneapolis

KEVIN M. TEEVEN. *A History of the Anglo-American Common Law of Contract*. (Contributions in Legal Studies, number 59.) New York: Greenwood. 1990. Pp. xii, 382. \$49.95.

The common law of contract achieved a central position in academic legal education in the late nineteenth century, and in America it was the vehicle for the introduction of the case system of instruction pioneered at Harvard University by Dean Christopher Columbus Langdell in the 1870s. Its doctrines and structures were viewed at that time as an expression of reason applied to ethical postulates; insofar as the history was of any significance, it merely illustrated the fact that getting the answers precisely right might take a long time. Today we lack such confidence, and a variety of conceptions of what general contract law is all about—or if it matters—compete in the marketplace of ideas.

One approach starts from the rather obvious point that the common law is a tradition. This method seeks understanding both of the modern institution of contract and, indirectly, of the institution of law in an analysis of its historical evolution and the ways in which that evolution has been presented and used by historians and lawyers. Kevin M. Teeven provides a comprehensive and necessarily concise history of a body of law that has evolved over the course of the last 800 years and also a guide to the extensive literature on the subject. Given the immense body of writing on contemporary law and the prominence of the subject in legal education, one might imagine that he would have numerous rivals, but this is not the case. Teeven's is the first book of any significance that attempts to trace the common law of contracts from its origins in England, through its arrival in America, and thereafter up to the present day, the emphasis in the second half of his book being on the United States.

Teeven begins with an account of medieval law before the development of a jurisdiction over informal contracts and of the rise of informal contract enforcement in the sixteenth century. Following is a chapter on the aftermath of the pivotal decision in *Slades* case (1602) and a discussion of the contributions of eighteenth-century legal thought, in particular the increased emphasis on equitable theories of contract and the development of commercial law particularly associated with Lord Mansfield. Teeven crosses the Atlantic to explore colonial contract law in the seventeenth and eighteenth centuries, and then moves into the nineteenth century, the era of elaboration of contract doctrine, which continues to dominate the field today. This was the period during which two receptions occurred, of civilian contractual doctrine and of intellectualized English common law. Chapter 7, titled "Modern Doctrinal Reformulation," concerns the doctrinal developments that have taken place since 1875, principally in the United States. The final chapter examines paternalistic intervention into

contract law motivated by a desire to protect weaker parties from the logic of the free market. Teeven's approach to the subject is doctrinal, and his starting point is that this branch of the law has had an intellectual life of its own, to a considerable degree insulated from social and economic forces. He gives a qualified support to the idea of the autonomy of law. If this is true, perhaps the reason is that the doctrinal elaboration of the law, which is both fascinating and important to lawyers, makes very little difference to the sum of human happiness or the gross national product; it is not so much autonomous as irrelevant. Teeven is aware of what his critics may say, but his concern is to make his subject accessible. He provides comprehensive references to the literature and writes clearly and readably. This is a valuable book.

A. W. BRIAN SIMPSON
University of Michigan,
Ann Arbor

PETER BUITENHUIS. *The Great War of Words: British, American, and Canadian Propaganda and Fiction, 1914–1933*. Vancouver: University of British Columbia Press. 1987. Pp. xviii, 199. \$27.00.

In a readable, self-effacing style, Peter Buitenhuis adds to the chronicles of the *trahison de clercs*, narrating how, from September 1914 on, Britain's war propaganda bureau suborned an entire generation of "pre-war" authors, artists, and scholars who began to produce propaganda rather than criticism, posters rather than art, fiction in the service of power rather than truth. With notable exceptions (Bertrand Russell, George Bernard Shaw, and Thomas Hardy) and for good fees many of the most respected authors in Britain (especially John Galsworthy, Arthur Conan Doyle, Ford Madox Huefner [before he went to France], Rudyard Kipling, H. G. Wells, Hilaire Belloc, John Masefield, Gilbert Murray, Arnold Toynbee, Lewis Namier, and Arnold Bennett) began to generate what Russell called the "foul literature of glory." Invariably with a sense of high purpose, a clear conscience, and occasionally with a sense of release and liberation, they abandoned the "critical," "detached," "oppositional" stance toward the powers-that-be expected of the man of letters.

At first glance Buitenhuis seems to exaggerate the effects on wartime society of this general enlistment of popular authors in the service of national belligerence. But his argument is convincing: the authority and respect accorded authorship was much higher before the war than after. Propaganda works by the literati were effective in forming a "moral majority" that would not hear of peace because Wellington House decided early to work unofficially, and secretly, as a paymaster and broker between individual authors and commercial publishing houses, eschewing any official imprimaturs. Prowar sentiment seemed to well up from below within the private

sector, enunciated by the most respected and substantial voices and pens. This sensitivity to the necessity of channeling effective propaganda through "society" in democratic countries explains something of the effectiveness of British efforts to turn the United States away from neutrality. Sir Gilbert Parker, in charge of stating the British case to Americans, distributed his materials through people prominent in American universities, churches, and the professions and claimed a network of 13,000 prominent citizens in 1916: "we have an organization extraordinarily widespread in the United States, but which does not know that it is an organization. . . . [In] the eyes of the American people the quiet and subterranean nature of our work has the appearance of a purely private patriotism and enterprise" (p. 18).

The importance of this literature of glory was enhanced too by the fact that, prior to the invasion of Belgium, no one knew why the war was being fought. The job of propagandists was to generate war aims and to legitimate the conflict as a war of civilization against barbarism. The official sponsorship of hate condemned any talk of peace as treason and pro-Prussian, suppressing much more than in Germany or Austria any opposition to the war. More significant was the "self-censorship" of the British press and the exclusion of war correspondents from the front, which created a situation where fiction substituted for news; myths and Victorian sentimental tales, set in an imagined domain of battle, could and did supply "information" to populations eager for news and consolation. The war of the fiction writers was fought by gallant "bronzed" self-sacrificing soldiers conducting bayonet charges across fields of honor hallowed by their blood and in resolute attacks led by resourceful commanders. This version was accepted by the home front as the real war, thus making it impossible for soldiers to describe their foul and humiliating reality. Ultimately, the news of conditions at the front leaked out, the enormity of the mortality became apparent, and the bleak continuation of war made victory seem much like defeat. In the last two years of the war the lies of the propagandists were slowly revealed as such, to the loss of literary reputations. "The younger writers, many of whom had served in the trenches, lost confidence in the authority of the written word and turned against their elders. The reading public no longer had the trust in important authors that they had in the days before the Great War. The prestige and power of authorship dwindled significantly" (p. xviii). The demise of the author was signaled during the war itself when, in 1916, Lord Beaverbrook and Lord Northcliffe shifted the emphasis from literary propaganda to the newer media: the popular press, film, and photographs. The age of the author was over and that of the anchor person had dawned.

Much of the power of Buitenhuis's narrative comes from the slowly accumulating detail that, despite his abstention from any overt moral judgment, makes his

narrative a rather damning indictment of the generation. By eschewing the detachment and alienation traditionally expected of the intellectual, the generation abandoned any possibility of "objectivity," forfeiting the basis of its moral authority. In retrospect we may doubt the reality of the autonomy of letters and regard the alienation of the intelligentsia as a fable debunked by war and by Marxists subsequently. Writers have always served the state, the church, the ruling class, whoever paid, and nowhere more effectively than when they pretended not to. But the cynicism of retrospect distorts the history of a culture perhaps unique in that its most respected theorists, from the Renaissance forward, spoke more often as critics and outsiders than priests or cheerleaders of power.

This is a useful and informative book, yet it is limited by substituting a moral judgment for a historical thesis. History is not a court where guilt or innocence is adduced from the evidence and judgment passed from the high seat of retrospect. It is an activity in which we try to find out what happened and to describe the constraints and incentives that influenced the choices of historical actors. In order to understand how authors in 1914 could welcome the war as an end to their alienation, aloneness, and powerlessness, one would have to have at least a consideration of the role and problem of authorship before the war. The treason of the intelligentsia in 1914 and subsequently in the culture produced by modern war is a historical fact. But what does this fact mean, of what complex is it a part, to what problematic does it relate? The judgment of "guilty," spoken or unspoken, prevents any further investigation of the phenomenon and indicates how deep and pervasive is the subordination of thought to political moralities that began with World War I.

ERIC LEED

Florida International University

KARL HUFBAUER. *Exploring the Sun: Solar Science since Galileo*. (New Series in NASA History.) Baltimore: Johns Hopkins University Press. 1991. Pp. xv, 370. \$39.95.

After four centuries of research, scientists have developed a sophisticated understanding of solar phenomena. Historian of science Karl Hufbauer has analyzed the significant observational and theoretical changes in "solar science," focusing on three themes that underlay these changes. The growing availability of diverse observational and theoretical tools, the emergence and institutionalization of the complex solar physics community, and the increasingly detailed picture of the sun's structure characterize the development of this science. Employing a wide range of published and manuscript sources, Hufbauer surveys the evolution of solar science into its current

status as one of the most vital disciplines in astronomy.

Although solar phenomena attracted the interest of astronomers throughout the seventeenth and eighteenth centuries, the perspective that guided early studies remained that of celestial mechanics. Galileo's study of sunspots, for example, suggested the mutability of the sun and provided important support for Copernicanism. Observational and theoretical shortcomings, however, delayed the emergence of a clear account of solar structure and evolution until the nineteenth century. Especially during the middle decades of the century, developments in spectroscopy and thermodynamics led physicists to abandon the planetary model of the sun and to replace it with a "heat-engine" model. Of equal importance was the creation of a separate "solar physics" community within astronomy, composed primarily of amateurs and "marginal professionals." More receptive to the new ideas and perspectives of the increasingly interdisciplinary field of solar science, these individuals laid the foundation for the dramatic growth of solar physics in the late nineteenth century under the guidance of George Ellery Hale. Hale's energetic devotion to the discipline led quickly to new theories, improved instruments, and an institutional base that included the Mt. Wilson Observatory and the *Astro-physical Journal*.

The nonspecialist may find explanations of certain pre-twentieth-century topics somewhat daunting, but Hufbauer discusses more recent contributions in a concise and accessible fashion. The 1920s and 1930s were marked by many theoretical and observational advances, the most important of which was the nuclear fusion theory of solar energy. The post-World War II expansion that characterized science in the United States led to several important advances, including the construction of solar observatories and the use of rockets as observing platforms. As expected from a title in a NASA history series, this work is especially effective in its discussion of space-age topics. Post-Sputnik expansion not only increased solar physicists' data base through space-based instruments but also spurred the subdisciplinary institutional growth that led to new divisions within scientific organizations and new journals such as *Solar Physics*. Hufbauer concludes his examination with extensive discussions of two examples of space-age research. Both solar wind studies and the investigation of solar variability owed much to space observations, but ground-based observers contributed significantly. The interdisciplinary nature of the field continued to be evident as well, with solar variability studies primarily the work of scientists involved in precision radiometry. These and other recent research projects show clearly the importance of outsiders to solar research and the enlargement of the discipline's research agenda in response to external initiatives.

Hufbauer provides an excellent survey of the

emergence of solar science, with appropriate emphasis on developments in the twentieth century. His clear description of the interdisciplinary nature of the discipline adds valuable perspective to this treatment of a leading twentieth-century topic. Solar physics may well have been a "beneficiary" rather than a "donor" science, but Hufbauer's account emphasizes that solar physicists made effective use of their colleagues' contributions.

GEORGE E. WEBB

Tennessee Technological University

SUSAN LEIGH STAR. *Regions of the Mind: Brain Research and the Quest for Scientific Certainty*. Stanford: Stanford University Press. 1989. Pp. xiii, 278. \$29.50.

Sociologists who study science and medicine increasingly are looking to the realm of past events as an arena to apply and even develop their explanatory models. The assumption is that data, after all, are data. Susan Leigh Star's account of the rise and proliferation of a particular school of brain research that flourished in the 1870s, the cerebral localizationists as led by J. H. Jackson at London's National Hospital for the Paralyzed and Epileptic, has been held as an exemplar in this regard. Star's research results from the crossing of two lines of interests. First, she is intrigued by the development of "an extremely successful theory about the nature of the brain" (p. xii), whose particular history poses a challenging puzzle. The ascendance of the localizationist school from "ridicule to prominence" (p. 4), with the corollary defeat of its opposition, the diffusionist school, is traditionally dated to a publication in 1906 by C. S. Sherrington. Star claims, however, that Sherrington was in fact more diffusionist than localizationist. How, then, did Sherrington become celebrated as the foremost localizationist? How also, she wonders, did the localizationist school manage to succeed in the face of a barrage of anomalies that sorely tested it?

For answers to these puzzles, Star turns to her second enterprise, the exposition of a particular sociological model for the analysis of scientific progress. Star claims that because scientists' theories are not disembodied entities but instead are embedded in the particular forms of their scientific practice, a solution will come not from an intellectual understanding of scientific ideas but rather a social analysis of work. Localizationists thus looked to Sherrington's contributions as the culmination of their efforts for the reason that his work, although scientifically and philosophically distinct, followed in close step with the exact sociological structure of their enterprise as a going concern. As to the second point, Star argues that the management of uncertainty is at the core of the scientific endeavor, uncertainty defined both in institutional and professional as well as in disciplinary terms. The localizationist school was especially suc-

cessful because it provided a model for action that was "plastic" and "coherent," both intellectually and sociopolitically, and was able to adapt to and incorporate challenges as they arose, whether in the laboratory or on the social stage. Star's primary motivation is to find a rapprochement between externalist and internalist models of scientific change, hoping to heal the rift that was set off by Kuhnian-based studies.

Is Star successful in her attempt to construct a retrospective sociology? The answer may depend on the reader's own particular disciplinary location. Sociologists may justifiably find her account interesting and stimulating. Historians, however, may remain unconvinced and troubled. As much as Star claims to base her analysis on the social history of work, there is in fact very little reconstruction of what was going on at Queen Square and elsewhere. We have too little information by which to compare the "going concern" of the diffusionists to that of the localizationists. Just as problematic, there is far too little re-creation of what was at stake socially and culturally in the competing models of brain and behavior.

In this regard, the importance of phrenology in promoting the material basis of the soul is hardly touched on. Some errors of fact and interpretation also surface (the EEG was not developed in 1910, but more than twenty years later; and the subsequent advent of psychosurgery was by no means based on strict localizationist theory.) Her core narrative also argues against itself. Star maintains both that Sherrington's model of the integrated nervous system provided the basis for the realization of the localizationist paradigm well into the current century, as well as the contrary position that Sherrington was not a true localizationist. And, lastly, by targeting her analysis primarily at Kuhnian models of scientific change, Star is engaging a historiographic battle that has already become somewhat dated. Indeed, one may wonder how much different her theory of plasticity and coherence really differs from Kuhn's conception of a paradigm operating in "normal science" conditions.

JACK PRESSMAN

*University of California,
San Francisco*

EUGENE RODGERS. *Beyond the Barrier: The Story of Byrd's First Expedition to Antarctica*. Annapolis, Md.: Naval Institute Press. 1990. Pp. xiv, 354. \$24.95.

This volume by Eugene Rodgers is certainly the definitive account of Richard E. Byrd's first expedition to Antarctica and a high-water mark for expedition histories. Thanks to radio broadcasting and the popular press, Americans could follow Byrd's expedition to a place of incomprehensible cold, seasons of endless light and dark, and caverns of dazzling blue ice. From Antarctica, amid crackling transistors, bright orange buildings, and stockpiled parts, Byrd

worked assiduously toward one goal: the first flight over the South Pole. In the eyes of the public, he became an American hero second only to Charles Lindbergh. Yet, as Rodgers notes, Byrd feared flying and did not operate the controls. Furthermore, he may have been less than sober on course and probably missed the pole.

Although his promised scientific reports never materialized, Byrd's costly venture ended well enough. Byrd's apparent success showered him with honor, including promotion to rear admiral, and wealth. The commander's glory, however, tells more about expectations and achievement than about daily routine. Of course, details of life on shifting Antarctic ice are far from humdrum.

Byrd exacted uncompromising loyalty from members of the expedition party. Keeping their personalities in balance was a test of leadership. Byrd juggled their skills, which ranged from meteorology and aviation to engineering and dog sledding. Besides technical experts, their number included a *New York Times* reporter, a lucky Boy Scout, and Byrd's beloved terrier Igloo. The days were marked by activities unique to a land of ice: assembling prefab equipment under conditions of congealing kerosene, shooting weaker huskies, cleaning the "crystal palaces" or latrines, and combating cabin fever. Days of night were met with heavy partying, reading (in Byrd's case Dickens and detective stories), poker, and films, including of all things *Nanook of the North*. Everybody was always a little thirsty.

The expedition returned to North America to the advent of "talkies," the financial crash, and an enormous amount of publicity. Byrd's aviation claims were pretentious, but he did not compromise the safety of his men. Despite his petty jealousies, the admiral appears all the more remarkable for his pretrip planning and shrewd, if sometimes secretive, management tactics. Rodgers's book is a tribute to Byrd's vision in those vast white spaces beyond the barrier.

CHARLOTTE M. PORTER
Florida Museum of Natural History
University of Florida,
Gainesville

ANCIENT

BARRY B. POWELL. *Homer and the Origin of the Greek Alphabet*. New York: Cambridge University Press. 1991. Pp. xxv, 280. \$80.00.

H. T. Wade-Gery proposed that the alphabet was invented in Greece to record verse (*The Poet of the Iliad* [1952], 12–14). Barry B. Powell argues that alphabetic writing was invented to record the *Iliad* and the *Odyssey*. Homer remains, for Powell, an oral poet, but his theory is supported by the discovery of more and more evidence and accounts over the past four decades that argue for the literary nature of the earliest inscriptions. The evidence uncovered after 1952 has

been highlighted by the publication of Nestor's Cup (1955) from a grave in Ischia and graffiti on shards from Lefkandi in Euboea, which may be the earliest examples of writing in archaic Greece (1980). Euboean connections with East (Al Mina) and West (Cumae, Pythekoussai) in the Greek world suggest that that island was the home of the inventor of the alphabet.

Powell attempts to answer the question, "Why was the Greek alphabet invented?" He sets out in great detail a thesis that builds on work presented in articles. His essay in *Kadmos* (1988) interpreted the inscription of the Dipylon oinochoe as a hexameter of bardic song, part of a second hexameter, and part of an abecedarium. In the journal *Classical Antiquity* (1989), he discussed all early inscriptions before 650 B.C. The three-line text of Nestor's Cup was shown to be a parody of Homeric verse, from an aristocratic symposium: a line of prose possibly imitating a proprietary inscription, followed by two hexameter lines in amplification of it.

Chapters 1 and 2 of his book provide background on writing before the Greek alphabet, aiming to show the societal needs met by earlier systems of writing. The alphabetic writing of archaic Greece, Powell demonstrates, represented the spoken language economically and unambiguously, whereas the logo-syllabic writing of Sumer and Egypt and the syllabic scripts of Cyprus, Linear B, and Phoenicia were suitable for the needs of highly centralized authorities, supported by a literate class of professional scribes.

Chapters 3 through 5 examine the earliest Greek inscriptions and Homer, concluding that the emergence of the Homeric poems and the invention of the alphabet occurred between 800 and 750 B.C. Powell discounts the possibility of commercial origin for many early texts: parts of names, owner's inscriptions, abecedaria. Powell's thesis must remain, in the present state of our knowledge, a demonstration of the possibilities inherent in an uncertain body of evidence, although it is carefully developed and presented in an interesting and compelling manner. He deserves the gratitude of classical scholars for his willingness to tackle a difficult subject and his forthright and comprehensive discussion of it.

WILLIAM C. WEST
University of North Carolina,
Chapel Hill

J. A. S. EVANS. *Herodotus, Explorer of the Past: Three Essays*. Princeton: Princeton University Press. 1991. Pp. x, 166. \$24.95.

This short volume comprises three separate essays, each of which treats Herodotus with respect as a writer of original talent and not as a mere gossip, all of whose notions came second-hand. None of the essays presents any startling new thesis or discovery,

but all are learned and will be read with profit by specialists and, to some extent, a larger audience.

The first essay, "The Imperialist Impulse," the most interesting and subtle in its approach, discusses causation. To what does Herodotus attribute the origin of the expansionist tendency in the Persian empire that led to the confrontation with Hellas and the focus of Herodotus's narrative? J. A. S. Evans argues convincingly against the view that vengeance plays a major role in Herodotus. He stresses the importance of the *nomos* or "disposition" of the Persians as an aggressive, imperialist power. That said, Evans also cites the role of individual choice. He begins with the rather limited choice that Gyges made to forestall his own death and murder his master, thus starting the chain of cause and effect that led to the ascent of Cyrus and eventually to Xerxes's expedition against Greece. As to reconciling *nomos* with choice, Evans avoids oversimplification: "We need not expect from Herodotus any solution of the dilemma of predestination and free will" (p. 37).

We might expect Evans to continue his analysis of the role of individual choice in the middle essay in the volume, "Individuals in Herodotus." Instead, we find an overview of Herodotus's biographical method together with brief sketches of each of a number of Persian and Greek personalities. Evans contrasts Herodotus's complex and in general sympathetic portraits of Oriental despots with the more problematical pictures of such Greek figures as Pausanias and Themistokles, the latter group colored by both hostile and friendly sources.

The final essay, "Oral Tradition in Herodotus," uses comparative ethnographic material, largely from Africa, to speculate about the ancient historian's oral sources and examines his book as itself partially an oral document. Some readers may feel that Evans here stretches the scanty ancient evidence, not much more than a few phrases in Pindar, when he argues that Greece, like Africa and Iceland in historical times, abounded in professional prose remembrancers, called *logioi*. Evans also makes Herodotus sound rather like a modern doctoral student in ethnology: "Having collected these various local traditions, he collated, verified, and amplified them by cross-checking, matching them whenever he could to the surviving evidence" (p. 110). Particularly in this final section, scholars may feel that Evans, despite many footnotes that cite specialist works, does not take sufficient account of some work by other scholars where it might inform his own ideas. For example, in a discussion of Herodotus's sources, he cites Detlev Fehling's book (admittedly controversial but recently translated into English) on a minor point (p. 136), but does not comment on Fehling's major thesis that virtually all named sources in the *Histories* are invented.

STEWART FLORY
Gustavus Adolphus College

KEITH R. BRADLEY. *Discovering the Roman Family: Studies in Roman Social History*. New York: Oxford University Press. 1991. Pp. viii, 216. Cloth \$32.50, paper \$11.95.

MARC KLEIJWEGT. *Ancient Youth: The Ambiguity of Youth and the Absence of Adolescence in Greco-Roman Society*. (Dutch Monographs on Ancient History and Archaeology, number 8.) Amsterdam: J. C. Gieben. 1991. Pp. xvi, 401. f. 130.

In two valuable contributions to the history of Roman family and life course, Marc Kleijwegt and Keith R. Bradley seek to define the differences between ancient and modern social relations and *mentalités*. Kleijwegt's book offers a polemic against the work of Emil Eyben, arguing that in classical antiquity youth were not considered an alienated group but were prepared for early entry into adult life, whether as doctors, lawyers, or municipal benefactors. Bradley presents previously published articles, revised and supplemented by new introductory and concluding chapters. His essays delineate the roles of slaves in the household as wetnurses and childminders and emphasize the weakness of the conjugal unit subject to frequent divorce and high probability of death.

In the construction of their theses and their use of evidence the books share certain features. Both repeatedly state that Roman attitudes toward family members and life course contrast starkly with those of today. Adolescence, with its connotations of alienation from the adult world, is an inappropriate concept for the ancient world, in Kleijwegt's view. For Bradley, "nuclear family" is a type too burdened by modern associations with conjugal romance, intimacy, and privacy to be useful in describing the Roman family: "Roman parents were not linked in egalitarian marriage, and their offspring did not rely exclusively on them for all their household and material needs" (p. 10). Seeking to document aspects of family and life course of little interest to Roman elite authors, both historians turn to inscriptions, especially tombstones, as a means of glimpsing aspects of life and strata of the population beneath the dignity of Roman high culture.

There is much to be said for the main arguments and the methods of these books. Roman family experiences undoubtedly differed from ours, insofar as the Romans did not have universal education to structure the early life course, and we do not live in households surrounded by slaves. Kleijwegt demonstrates that Roman youths could acquire education and enter into professional life before the age of twenty. Bradley emphasizes the diffusion of familial sentiment beyond the nuclear family to the slaves and kin who nurtured children whose parents usually divorced or died before raising them to adulthood.

Yet the sources and theses raise problems not always fully discussed by the authors. Kleijwegt's focus on inscriptions attesting professional and political careers gives little attention to the mass of the

population, for whom economic necessity must have been the incentive for early entry into working life. On this subject, Bradley's chapter, "Child Labour in the Roman World," is far more illuminating. Bradley's lists of funerary inscriptions mentioning wet-nurses, childminders, and unrelated *tatae* ("daddies") and *mammae* ("mommies") also may yield a partial and unbalanced view despite his occasional general cautions. These commemorations fill out our picture of the family, yet the reader ought to be reminded that fathers and mothers outnumber those other, more marginal figures in funerary dedications by more than ten to one. Nor is it simply a matter of numbers. For a qualitative comparison, one would do well to read Pliny's descriptions of his wetnurse and of his strong attachment to his mother (*Epistulae* 6.20.11). The comparison does not yield a sense of "the physical and perhaps emotional distancing of parents and children" (p. 29).

My primary concern about both books lies in the way their theses are constructed: in order to heighten the contrast between ancient and modern, they simplify modern society so that one may lose sight of the common features and problems faced by families then and now. According to Kleijwegt, "maybe the greatest difference between modern adolescence and preindustrial youth is that although both are separate from adulthood in that they are different, the former is totally devoid of adult characteristics. The adolescent is kept in seclusion at school so that ultimately he may be swiftly propelled into adulthood for which he is not fully prepared" (p. 43). Kleijwegt is at pains to explain why ancient youths "did not preach revolution" (p. 67). This is to problematize the issues in an odd way. The rebellious adolescents of the 1960s and 1970s are the anomaly to be explained, whether in contrast to ancient youths or to the complacent youths of the 1980s who (at least in the United States) are not "secluded" but are preoccupied with entry into the adult work force, often starting work in their teens.

Bradley hammers away at "conceptions of the Roman family that focus exclusively on the core unit of father, mother, and children" (p. 29). Although conceding that the Romans had an ideal of conjugal love and that parent-child relationships "may have been the most significant bond," he insists that "there was a constant tension between ideal and reality, in the sense that the former could not be guaranteed in view of the unimportance of affection and emotional compatibility in the latter" (pp. 92, 128). These conclusions are no doubt true, but perhaps less revisionist than the reader is led to believe. In what society has there not been such a tension? Where has the family focus been "exclusively" on father, mother, and children? Not the modern family, as Bradley realizes. All those he cites for characterizing the Roman family as "nuclear" (J. A. Crook, Suzanne Dixon, Beryl Rawson, Brent Shaw, and myself) have written about slaves in the household or the influence of kin on

important family decisions such as marriage. This is more than just a quibble about emphasis or originality. The urge to discount the nuclear family sometimes leads to small distortions. In contrasting the "often close and enduring" relationship between children and their slave nurturers with the "widespread familial dislocation" caused by parents' divorce or death, Bradley neglects to say that the child-slave bond was subject to the same disruptions through death of the slave, or the death or divorce of the slave's master, which could also lead to the breakup of the household. More generally, in the matter of childminding, the differences between ancient and modern are less stark than presented in Bradley's ideal type of the modern nuclear family. Obviously, the reality today is that many children are placed in child care outside the home for most of their waking hours by parents who are not less attached to them for it.

Kleijwegt and Bradley have filled out our view of Roman family life and life course in significant ways, but both illustrate the pitfalls of the history of *mentalités* in this field where attitudes are so complex and varied. The inclination is to sharpen the contrasts between ancient and modern views of youth or marriage beyond what can be justified by the diverse evidence.

RICHARD SALLER
University of Chicago

KENNETH S. SACKS. *Diodorus Siculus and the First Century*. Princeton: Princeton University Press. 1990. Pp. xii, 242. \$29.95.

Diodorus's *History*, originally entitled *Bibliothèque* or *Library*, comprised forty books of which fifteen remain intact; the rest survive only in excerpts and quotations. The book was a general history, principally of Greece and Rome, from the creation of the world to 60 B.C. Not surprisingly, it depended heavily on previous historical works. The standard view has been that the *History* was a rather mindless compilation from these earlier works, a view favored particularly by source critics who believed that passages from now lost historians could be lifted entirely from the pages of Diodorus. It was also believed that non-narrative portions of the work, such as the prefaces that introduce most books, had also been copied, virtually verbatim, from Diodorus's predecessors. The standard view has not lacked its critics, but Kenneth S. Sacks's book constitutes the most comprehensive and methodical assault on it.

Sacks's method is to examine the *History* for language, views, and attitudes that can be attributed with some confidence to Diodorus himself and to show that they occur throughout the narrative and non-narrative portions of the work. He argues for a similarity of language between the initial preface and the rest of the work; for example, *pleonexia* (greed) is

a theme recurring throughout the work (nearly fifty times), and the metaphorical sense of *metropolis* occurs outside Diodorus only in the works of philosophical writers (p. 21). Similarly, Diodorus's view of historical causality is shown to be influenced by the recurring themes of benefit, chance, and imperial decline (chap. 2). The significance attached to moderation (*epieikeia*, *philanthropia*) in the maintenance of empires is shown to be a Diodoran trait (pp. 42 and following). In chapter 3 Sacks maintains that necessity and benefaction, although not absent from other authors, constitute a Diodoran explanation for progress and notes Diodorus's interest in culture heroes. Sacks also counters some arguments espoused by the standard view. He argues convincingly that in the few cases where cross-references in Diodorus are unfulfilled, the reason is more likely to be a mistake on Diodorus's part than unintelligent copying of a source. He also suggests that the critical tone toward Rome in passages culled from Posidonius points to the intrusion of Diodorus's attitudes (pp. 153 and following).

Sacks is an honest critic who does not exaggerate the solidity of his conclusions. He certainly throws the standard view into question, even if it remains a problem that in some cases Diodorus himself is the main source for the authors with whom he is being compared. Sacks is justified in claiming, however, that his view "will cast a different light" on the account of the First Sicilian Slave War (p. 144). Whether he is right about Diodorus's hostility to Octavian and the effect of that on the *History* is more open to question. Despite Octavian's harshness toward Sicily, Diodorus's homeland, how strong was Diodorus's local patriotism likely to have been in the late 30s toward a city he had left in 60 B.C.? Nonetheless, in this thoughtful, densely written book, a strong case has now been made for Diodorus's independence as a critic.

G. M. PAUL
McMaster University

DAVID S. POTTER. *Prophecy and History in the Crisis of the Roman Empire: A Historical Commentary on the Thirteenth Sibylline Oracle*. (Oxford Classical Monographs.) New York: Clarendon Press of Oxford University Press. 1990. Pp. xix, 443. \$110.00.

The thirteenth poem in the collection known as the *Sibylline Oracles* is one of very few contemporary narrative sources bearing directly on the political and military history of the Roman empire during the middle decades of the third century. It contains 173 lines, most of them obscure and studded with textual difficulties; an unknown number of lines have been lost in a lacuna near the start. David S. Potter gives a detailed exegesis which both proceeds from and strongly confirms the hypothesis that the surviving poem reflects three main stages of composition: lines 1–88 are a mixture of material from different hands

and dates in the 240s; they were perhaps collected by the poet who composed lines 89–154 under the regime of Uranius Antoninus of Emesa in the summer of 253; and lines 155–171 were added ca. 265, before the death of Odaenathus of Palmyra. (The last two lines are a rather colorless epilogue.) Hence the poem is a composite work reflecting the attitudes of two Syrians who wrote in 253 and ca. 265. It expresses violent hatred of the Persians and deep admiration for both Uranius Antoninus and Odaenathus. Its authors were upper-class pagans at home in traditional Greek culture and loyal subjects of Rome.

The core of Potter's study comprises a new critical Greek text with English translation and a detailed discussion of textual and historical problems. An introduction almost as long as the commentary itself provides a succinct description of the economic, political, and military situation of the Roman empire in the third century, a survey of the historiography of the period, an account of the role of Sibylline oracles in Greco-Roman antiquity, and an analysis of the composition of the twelfth and thirteenth *Oracles* in the surviving collection.

The book began as an Oxford D.Phil. thesis, but it has grown into an impressive and mature piece of historical scholarship. In particular, the opening chapter contains both a reliable narrative of the political and military history of the period from Severus Alexander to Aurelian and a fine analysis of the main structural features of the "crisis of the third century"—a term which Potter explicitly defends as an appropriate characterization of the period 235–284 (p. vii–viii). If nonspecialist readers find the detailed commentary hard going because Potter continually quotes Greek and Latin at length without providing an English translation, they can be assured that this feature is fully justified by the nature of the undertaking: the reporting and assessment of evidence—archaeological, papyrological, and epigraphic as well as literary—maintain a high standard of accuracy and fairness.

The treatment of historiographical problems does not show quite the same sureness of touch. Some of Potter's conclusions unhappily recall the excesses of *Quellenforscher* of an older generation: he holds, for example, that the author of the *Historia Augusta* never consulted either Dexippus or the extant Herodian directly but always through the intermediary of an otherwise unattested "reasonably extensive Latin history that was the source of the *Historia Augusta* biographies after Alexander Severus" (p. 72). Fortunately, however, questionable source analysis does not seriously impair the value of Potter's historical commentary. His work will provide a secure basis for all future enquiries into the period about which the thirteenth *Sibylline Oracle* was written.

T. D. BARNES
University of Toronto

R. A. MARKUS. *The End of Ancient Christianity*. New York: Cambridge University Press. 1990. Pp. xvii, 258. Cloth \$49.50, paper \$12.95.

In this volume, R. A. Markus has further developed themes he delineated in his well-received *Saeculum: History and Society in the Theology of Saint Augustine* (1970; 2d. ed., 1988). The central question for Markus is: What took place in the two centuries between Augustine of Hippo's time and that of Gregory the Great (pope, 590–604) that caused the expressions of Christianity in Gregory's era to be so different from those in the age of Augustine, Ambrose, and Cassian, three church fathers whom he admired and whose ministries he saw as far superior to his own (p. xi)?

The age of Augustine was marked by the consolidation of Christianity's position of centrality in the empire after periods of persecution. The Christians were no longer an insignificant minority. Markus sets the pastoral context of increasing nominalism facing the bishops of the late fourth and early fifth centuries. Augustine, Ambrose, and Chrysostom all addressed this issue and Augustine concluded that there would always be tares mixed in with the wheat and that the Church was not living in "eschatological purity" (p. 52). He found himself in conflict with perfectionism, whether from the Donatists or the Pelagians. The basic issue was: What force divided the human race into those who were close to God and the rest? His answer was grace; his opponents believed it was a purity sought in perfection here on earth.

Monasticism developed in this context and became the dominant force in the church by Gregory's era. Markus sees the seeds of the change already at work in Augustine's era and traces their development to their more mature expression in the late sixth and early seventh centuries. He also demonstrates that the changing consciousness regarding martyrs, holy places, and the place of the congregation versus the church building show these forces at work.

Markus's heavy debt to Peter Brown is acknowledged in the dedication and evident in the notes. Markus's exposition of the intellectual history of the period, however, expands observations in Brown's definitive biography, *Augustine of Hippo* (1967). If, in the current generation, Brown is Augustine's most significant biographer, then Markus has made the greatest strides in putting the saint in his intellectual context and evaluated Augustine's contributions most cogently. Future scholars may wish to focus some of Markus's observations, and the transition from the Christianity of the ancient world to medieval Christianized culture has not been fully articulated. This study is a significant contribution and will, undoubtedly, be cited often in further discussions of these issues; it is essential to academicians and libraries with interests in this area of study.

R. A. KRUPP
Western Seminary
Portland, Oregon

MEDIEVAL

FREDERICK S. PAXTON. *Christianizing Death: The Creation of a Ritual Process in Early Medieval Europe*. Ithaca: Cornell University Press. 1990. Pp. xiv, 229. \$31.50.

In this insightful book, Frederick S. Paxton has traced the development of the rituals for the sick, the dying, and the dead in Christianity from their origins in the fourth century through their decisive reorganization in the Carolingian ecclesiastical reform. On one level he provides a useful synthesis and interpretation of the earlier textual researches of such liturgical historians as Cyril Vogel, Jean Deshusses, and Damien Sicard. The importance of the interpretation should be stressed, for there is much here that is new (for example, the identification of Benedict of Aniane as the author of an important rite for anointing the sick). But the book's real novelty and importance lies in Paxton's use of insights gained from social history and anthropology, particularly from Arnold van Gennep's model of rites of passage, to illuminate the meaning, rather than merely the structure, of these rituals. Paxton never loses sight of the fact that these rituals served to separate dying Christians from earthly society and to incorporate them into the kingdom of heaven. Few books have so successfully conveyed the concrete nature of the link that existed in the minds of medieval Christians between the kingdom of heaven and that of this world.

The book is divided chronologically into five chapters, respectively treating the late Roman empire, the early Germanic kingdoms (particularly Merovingian Francia, Visigothic Spain, and Celtic Ireland), the "high reform" of the Carolingians in the late eighth century, the work of Benedict of Aniane, and the final synthesis of the Carolingian reformers in the later ninth century. Paxton traces the replacement, in deathbed rituals, of the original Roman focus on the fate of the soul with Germanic and Celtic concerns with the needs of the dying person. Penance became ever more important and the community that surrounded the dying person became ever more involved in the process, developments which led to the invention of such new forms of post-mortem commemoration as confraternities of prayer and *libri memoriales*. Where the Roman rituals had once joyously celebrated the entrance of the soul into paradise, later rituals betrayed the fact that "death held more terror" (p. 202) for the newer Christians of northern Europe.

In rituals of healing a tension was similarly to be found between two competing models: the first, found in Visigothic Spain, attempted to return a person to physical health; the second, developed in Ireland, stressed spiritual rather than physical health and saw illness as a preparation for eternal life. Over the course of the Carolingian reform these "crosscurrents" came to be synthesized into a single ritual process that "accompanied final illness, death, and

burial in a coherent series of ritual moments . . . [giving] shape and voice to all the streams flowing into the Christian culture of the north—Roman, Gallican, Irish, and Visigothic" (p. 207). This stood as the basis of the liturgies for the dead in the later Middle Ages and beyond.

Paxton has succeeded in composing an account of these ritual developments that should prove to be standard. In his insistence on the importance of the social historical context, as when he treats the rituals for anointing the sick as but one among many competing strategies for dealing with illness, he has also provided a model treatment of the history of religious practice. If he has been less concerned to relate practice to changing beliefs about the nature of sin and the afterlife, such theological issues may legitimately be said to fall beyond his chosen scope.

As is almost inevitable in a book that relies on the description of numerous individual manuscripts, the presentation of the data and the abundance of the quotations from individual rituals occasionally becomes overwhelming. But Paxton's clear and uncluttered writing style, as well as a number of helpful schematic charts that detail the most important rituals, keep confusion to a minimum. This attractively produced and impeccably proofed volume deserves a place in college and university libraries. It is of obvious significance to specialists in liturgical history and in the early Middle Ages. More broadly, however, it will also appeal to historians of medieval Christianity and to scholars in the growing field of ritual studies.

THOMAS HEAD
Yale University

MARIA ELISABETH WITTMER-BUTSCH. *Zur Bedeutung von Schlaf und Traum im Mittelalter*. (Medium Aevum Quotidianum Sunderbund, number 1.) Krems, Austria: The Society. 1990. Pp. 400.

Dreaming is a universal experience of humankind. Accounts of dreams loom large in written sources from medieval Europe, although scholars are often uncertain how to understand them historically. In the Middle Ages, the origins and interpretation of dreams were of great interest to theologians, medical doctors, ordinary clerics, nuns, and lay people. Opinions about where dreams originated and what they meant were quite diverse. There was a rationalistic tradition that connected dreaming to such causes as digestion, the stresses of daily life, and the symptoms of physical illness. When dreams were analyzed from such a perspective, they were regarded as without significant meaning. There was, however, another tradition of thought that saw dreams as events calling out for interpretation because they might be messages from God, diabolical trickery, or predictions of the future. This interpretation of dreams presented moral and intellectual problems for medieval Chris-

tians. On the basis of certain Old Testament texts (for example, Leviticus 19:26), the interpretation of dreams was forbidden as a form of sorcery. On the basis of other biblical texts (Matthew 2), some Greco-Roman texts, and deep-rooted cultural practices, the effort to interpret dreams was defended.

During the early Middle Ages, Christian intellectuals were skeptical of attempts to interpret dreams. Many clergy and laity, however, sought to find meaning in their dreams. As Greco-Roman-Arabic astrology gained prestige during the thirteenth century, a more tolerant attitude spread in intellectual circles, although official disapproval of efforts to interpret dreams never disappeared.

This book by Maria Elisabeth Wittmer-Butsch is an encyclopedic treatment of sleep and dreams in medieval Europe; all geographical areas and the period from 500 to 1500 are included, as well as medical, psychological, religious, social, and political aspects. There are interesting insights, but they are not followed up in a systematic way. Because of its remarkable breadth, the book has an element of superficiality. The author has read widely in the sources—perhaps too widely—and attempts to encompass every conceivable problem in 370 pages of text. Wittmer-Butsch is also familiar with modern research on sleeping and dreaming, although the findings of that research are not well integrated into the work. There is a disturbing element of presentism in the approach. For instance, medieval thinkers whose ideas seem to anticipate in some aspects those of Sigmund Freud or Carl Jung are praised for being precociously modern. On the basis of psychological research, the author is relatively confident that she can distinguish accounts of genuine dreams from literary ones. In spite of some cautionary language, there is an uncritical assumption that the categories of modern dream analysis apply directly to medieval dreams. Even more problematic is the implicit assumption that all medieval dreams, whether from Germanic or Mediterranean dreamers or from the sixth or sixteenth centuries, can be treated alike.

In spite of abundant documentation, the book does not reach its potential for illuminating a rich area of inquiry. It is regrettable to note that the book was bound in such a flimsy way that pages came loose during its first reading.

JOSEPH H. LYNCH
Ohio State University,
Columbus

DAVID ROLLASON. *Saints and Relics in Anglo-Saxon England*. Cambridge, Mass.: Basil Blackwell. 1989. Pp. xii, 245. \$39.95.

In spite of its title, this highly informative historical survey does not confine itself to the saints and relics of Anglo-Saxon England; rather, David Rollason sets his material at the center of a wider-ranging historical

and geographical study. Thus, a short part 1, entitled "Origins," treats the early Christian martyrs and the evidence for saints' cults in Roman Britain, and part 3 extends to the year 1100 to include the impact of the Norman conquest. This inclusive perspective, as well as logical organization and exceptionally lucid writing, make this book an excellent overview of its subject and a fine introduction for students or scholars new to the field.

On the one hand, Rollason's book represents the traditional historical survey, dealing with a subject chronologically from its origins up to a specific date. On the other hand, his integration into English social and political history of a subject too often treated as peripheral and of interest only to historians of religion gives hagiographic materials the central place that scholars have more recently been claiming for them. The reader is led to the unstated but compelling conclusion that it is impossible to understand Anglo-Saxon England without knowledge of its saints.

Extensive use of primary sources, including visual material, is combined with judicious interpretation and frank acknowledgement of controversial positions, enabling readers to find their bearings in the field rather than to uncritically adopt Rollason's positions. Rollason's story is one of continuity, from the cults of Romano-British saints, which may have had a significant impact on later English Christianity, to the fostering of Anglo-Saxon saints' cults by Norman bishops. It is also one of downward expansion in society, from "a narrow circle of royalty, church and nobility" (p. 97) in the period to about 850, to lay people, including people of low status, in the subsequent period. The multifaceted relationships between saints and royalty receive special attention, including, for example, the intriguing suggestions that the church may have "used the cults of murdered kings and princes to prevent assassinations" (p. 128), and that cults of relics may have played a role in the political unification of England (p. 158).

Rollason uses evidence about the cults of saints and relics to locate English practices within a wider European context. For the early period he explores the importance of relics from the Continent and "the pattern of direct influence from Gaul combined with Roman influence mediated through Gaul" (p. 72) in the practices of relic elevation and translation, in the forms of shrines, and in hagiographical writing. By contrast, analysis of the lives of the sainted churchmen who led the reform movement in tenth-century England leads to the conclusion that the major sources for the reform lay in Germany. And in treating the impact of the Norman conquest, Rollason admits his previous adherence to the view that the Normans disparaged English saints and generously acknowledges the importance of Susan Ridyard's work in changing this interpretation.

Based on his many important specialized studies, Rollason's book effortlessly synthesizes this work into

a highly readable and engaging introduction to Anglo-Saxon saints and relics.

PAMELA SHEINGORN

Baruch College

*Graduate School and University Center,
City University of New York*

LISA M. BITEL. *Isle of the Saints: Monastic Settlement and Christian Community in Early Ireland*. Ithaca: Cornell University Press. 1990. Pp. xvi, 268. \$28.95.

"Ireland was odd in the early middle ages," wrote Kathleen Hughes in 1973 ("Sanctity and Secularity in the Early Irish Church," *Studies in Church History* 10 [1973], 21). Since then a new generation of historians has discovered the affinities between Ireland's very oddness and the rest of Europe. Lisa M. Bitel's book is about boundaries, portals, links, and barriers, not least those between insular and Continental historians. Her themes, ingeniously explored, feel familiar to medievalists raised on the work of anthropologists like Victor Turner and historians like Peter Brown.

The book is organized as a spiral, beginning at a physical and spiritual center—the monastic sanctuary—then moving gradually outward to the wider world of which the monastery of A.D. 800–1200 was a part. The overriding theme is the importance of community for Irish monks and their neighbors. Hence the book opens with the continuity between monastic and secular settlements. The relics of saints, housed in the most central and privileged place in the monastery, were suns orbited by social groups of receding holiness: the monks, keepers of the relics, lived nearest the center; a few privileged nobles and kings, often related to the monks, were allowed occasionally to penetrate into the sacred space; in a more distant orbit, clients of the monks (both those who tilled the fields to keep the monks well fed and those of a loftier class who offered legal and military assistance) lived in a penumbra of spiritual protection and participation.

Situated beyond each monastery was the wider world of lay lords, petty kings, and other competing monasteries. These too were woven into a web of relationships. Monastic communities might ally themselves with a particular king, but when fighting broke out they acted as intermediaries between feuding princes. They tried to outweigh the temptations of war by raising fears of punishment; they cursed the violent and prayed for their defeat. The monks tried to control kings and impose on royalty their relic-centered notion of power.

When two monasteries competed, a more delicate "war" took place, based on the laws of hospitality. In this battle, Irish monks carefully calculated the social gains and losses implicit in feasting and fasting. The rituals of hospitality revealed distinctions in existing social statuses, but they could also be manipulated to suspend or invert those statuses; in every case, they

set up ongoing relationships. The bonds of community were so important that monastic exile and pilgrimage, however often invoked as ideals, were rarely realities. The book's epilogue covers the partial destruction and partial adaptation of this island society under the impact of the Norman invasions of the 1170s.

The question must be asked: how do we know about the pre-Norman society invoked here? The sources on which this book largely rely are Irish and Latin saints' lives, but they are not clearly from the period they are used to illustrate. Some of the lives most surely dated, such as that by Adomnán (d. 704), are too early. Most of the other lives, their authors anonymous, are perhaps too late. The so-called *Codex Salmanticensis*, a carefully arranged compendium of lives, was probably drawn up in the fourteenth century by English or Anglo-Norman monks. Other lives, both in Latin and Irish, exist only in still later manuscripts. Charles Doherty and Kathleen Hughes have traced elements of these lives to the eleventh or twelfth centuries. But other historians, such as David Herlihy, have used the same corpus of sources to illustrate pre-ninth-century Ireland. For other purposes the lives could be used equally well to discuss the sensibilities of the fourteenth and fifteenth centuries. Moreover, each layer of these much reworked lives represents a hagiographic witness awaiting interrogation: does the hagiographer evoke a society long gone or one present before him? Or does he suggest an ideal that never existed at all? The complex of problems posed by these sources are so delicate and so important that one wishes Bitel had devoted a chapter to their nature and her use of them.

Yet, even lacking such a chapter, this is a sensitive, thoughtful book, filled with wonderful insights useful even for historians far beyond the Irish Sea.

BARBARA H. ROSENWEIN
Loyola University
Chicago, Illinois

ANNE LOMBARD-JOURDAN. *"Montjoie et saint Denis!" Le centre de la Gaule aux origines de Paris et de Saint-Denis*. Paris: Centre National de la Recherche Scientifique. 1989. Pp. 392. 240 fr.

Anne Lombard-Jourdan's book begins with an inquiry into the place of the martyrdom of St. Denis, apostle to the French, who was martyred by beheading in the mid-third century. The issue has been complicated because the earliest accounts of the martyrdom, the *Vie de S. Geneviève* (520) and Gregory of Tours' *History*, come three centuries after the events they describe; moreover, as the legend was retold throughout the Middle Ages, details were added whose reliability is hard to judge. But setting the geography of the martyrdom of St. Denis is only a small part of the book. Lombard-Jourdan's broader

purpose is to understand how Paris, an inconsequential city under the Romans, achieved its importance during precisely the period when the legend of St. Denis was being elaborated.

Lombard-Jourdan's first task is to clarify the medieval historiography and relate it to the geography of the region. She shows that the earliest accounts all agree that Denis was executed on a small mountain near Paris, that he then walked with his head a distance a bit over a mile, and that his body was later moved to the site of the present basilica of St. Denis. Confusion about the precise site began somewhat later. Writing in the ninth century, for example, Abbot Hilduin of St. Denis took the name Montmartre to indicate that Denis's decapitation had occurred there—an error, because the name of the hill in fact derived from the Roman god Mercurius; Hilduin then added the detail to the legend that it was priests of Mercury who had decapitated St. Denis. Lombard-Jourdan, however, demonstrates that on the plain of Lendit near Paris there exists a small rise that was known as Montjoie as late as the eighteenth century; the location of this Montjoie is consistent with the earliest accounts of St. Denis's death and burial.

Lombard-Jourdan next places the legend of St. Denis into the context of the early Christian policy of attempting to associate Christian saints with places already sacred to pagan religions. She argues that the plain of Lendit was sacred before the Romans arrived, central to the tin trade but also the place at which assemblies of the Gauls gathered; the Romans had then appropriated the place for their own gods. Thus, St. Denis's martyrdom did not make the site where it occurred sacred; it claimed an already sacred place for Christianity. By extension, the Frankish battlecry "Munjoie," already known in the Song of Roland, apparently derived from the Germanic word *mund-gawi*; it was therefore apparently an invocation of protection rather than a direct reference to the legend of St. Denis.

The merger of these elements was finished by the monks at St. Denis, who completed the Christianization of the practice of healing and protection that had long been localized on the plain of Lendit. Moreover, it was as the closest town to St. Denis that the minor Roman town of Lutetia began its evolution into the medieval capital of Paris. Although Paris had been a favorite residence of Clovis and the later Merovingians, the Carolingians abandoned it for other towns. But after the Capetian counts of Paris claimed the title of king, their association with Paris, and its proximity with St. Denis, helped turn the town into the "caput regni." Significantly, this term was first applied to Paris by Abbot Suger of St. Denis.

Lombard-Jourdan's work is thus a study of the *longue durée* of the Paris plain. Many handsome illustrations and maps increase its appeal.

CHARLES M. RADDING
Michigan State University

ÉTIENNE HUBERT. *Espace urbain et habitat à Rome du X^e siècle à la fin du XIII^e siècle*. Foreword by PIERRE TOUBERT. (Collection de l'École française de Rome, number 135/Istituto storico italiano per il Medio Evo, nuovi studi storici, number 7.) Rome: École française de Rome and Istituto storico italiano per il Medio Evo. 1990. Pp. viii, 396.

Étienne Hubert has written a fundamental work for the understanding of the growth of the urban structure of Rome between 900 and 1300. He gives a full account of the unpublished and published sources available for the study of this structure, the most productive of which are the ecclesiastical archives and, to a lesser degree, independent family records and notarial books. In a thirty-five-page bibliography of the secondary literature, he presents the historiography and archaeology that may have any bearing on the subject. On this basis, his presentation proceeds in three parts: "The Urban Space," "The Urban Habitat," and "The Economy of the Real Estate."

In the first part, Hubert traces the beginnings of post-antique Rome between the ninth and eleventh centuries, the filling in of the Tiber Bend between the eleventh and twelfth centuries, and the accelerated expansion after the middle of the twelfth to the end of the thirteenth century. Through the eyes of the notaries, he sees the subdivisions of the urban space in their own time, not the classical "12 or 14 Regions" or the "Seven Churches of Rome" but a complex net of *rioni*, neighborhoods, and parishes. The results of his analysis are tabulated in an appendix. Also important are his observations on street patterns and identification of the main arteries.

The second part of the book avoids the picturesque, furnishing instead the cell structure of urban life with illustrations of typical building forms. The terms *curtis*, *domus*, *domus terrinea*, *turris*, *palatium*, and others are shown in their structural and social significance. The craftsmanship and materials of the building trade are given with particular attention to the relative importance of wood, masonry, and stone, and the exploitation of the remains of ancient buildings. He makes good use of notarial instruments for the reconstruction of family life, showing how the upper classes lived together as extended families while the common people typically lived as a nuclear family. He accomplishes this feat without the benefit of census records.

The third part of this volume is more problematic than the other two because of its reliance on ecclesiastical sources. The needs of churches and monasteries and the employment and administration of their real estate were different from those of big lay proprietors. Hubert emphasizes here, as in the first part, the important role of churches and monasteries in real-estate development in Rome. He notes a decline in development from the late eleventh to the middle of the twelfth century, followed by the opening up of new quarters through the parceling out of uniform

house lots. He differentiates between areas of lay and ecclesiastical expansion and offers a map indicating their distribution. Hubert finds significance in the practice of leasing the soil for the construction of a building by the tenant. In the ninth and tenth centuries these were long-term, renewable contracts: nineteen to thirty-nine years. From the latter part of the eleventh century onward, however, increasing numbers of them became very long-term, emphyteutic, leases. Hubert compares the costs of initial leasing, *entratura*, renewal, and annual rent. In the last chapter he tries to trace, as much as is possible, the changes in the real-estate market between the eleventh and fourteenth centuries, demonstrating here too a stagnation between the late eleventh and the mid-twelfth century.

Except for a brief reference to the Investiture Conflict and to the schism of 1130/1138, Hubert does not relate his findings to the "histoire événementielle" or to the wider ecclesiastical and economic history of Italy and Europe. But his book furnishes rich material for the work of others in these directions. His findings are readily accessible through his lucid presentation and his charts and maps. Because of its length, a systematic rather than alphabetical bibliography of secondary works would have been preferable.

REINHOLD SCHUMANN
Boston University

SOPHIA MENACHE. *The Vox Dei: Communication in the Middle Ages*. (Communication and Society.) New York: Oxford University Press. 1990. Pp. 353.

Like the medieval propagandists she has studied, Sophia Menache takes familiar ideas and gives them a new imperative through her use of communications theory. The result is a book both intriguing for its reinterpretations and frustrating for its reliance on generalizations.

Menache examines the creation of communication systems in England and France from the eleventh to the fourteenth centuries. Her premise is that communication is as much a product of historical milieus as technology. Her chosen milieu is the "social, economic, and political integration" that occurred in Europe after 1000 when, according to Menache, traditional feudal society declined and individuals were liberated from the corporations (extended kin-groups, rural villages, local religious communities) that both supported them and restricted their contact with a larger world (p. 3). When the population grew and people began to travel to cities, they became more isolated from old support groups and more vulnerable to propaganda, first from a centralizing papacy, then from nationalizing kings, and finally from heretical movements that wanted to challenge the new authority of both church and state. Churchmen were the innovators in developing the themes

and methods of propaganda, particularly the use of *vox dei*—the claim to represent the authority and will of God—which kings and heretics then appropriated and adapted to their own purposes of forming public opinion.

Menache's thesis may be more surprising to those in the field of communications studies than to historians. Most medievalists already believe, at the insistence of the *Annalists*, that the period after 1000 was not a period of stasis. We are also easily persuaded that modern communications were born then: that medieval popes and monarchs had a savvy grasp of the use of slogans, symbols, and stereotypes, and that the medieval public was cleverly becoming sensitive to the possibilities of political communication.

This is, however, merely a framework on which Menache hangs a mass of smaller reinterpretations that are often fascinating. Her description, for example, of the development of French and English stereotypes of each other during the Hundred Years' War is instructive and amusing. Yet her account of the cult of death fostered by the postinvestiture church and of the internal struggles of the Franciscans are syntheses of other people's arguments made to fit her thesis. How can she blame the Franciscans for a communications failure in comparison to Waldensians and Cathars? Where are Waldensians and Cathars today? Also, her demonstration of the successful and coherent purpose of church leaders, kings, and heretics leaves little room in her analysis for the great masses who held "public opinion"; she tends to disregard the variety of spiritualities, as well as the class, regional, and gender differences, that existed among both the people and their religious and secular leaders.

Nevertheless, even in simplifying and rehearsing well-worn material, Menache's knowledgeable use of the chroniclers and religious men of letters keeps us interested in her book. It is always useful for historians to take an outsider's perspective of a situation grown too familiar, just to make sure we have not missed anything important. Menache, who is both outsider and skilled insider, communications specialist and medieval historian, blends both perspectives in this piece of propaganda that, ultimately, succeeds in being persuasive. And, for those who enjoy Menache's book, the series of which it is a part contains other volumes of potential interest, such as Tamar Liebes and Eliha Katz's *The Export of Meaning: Cross-Cultural Readings of "Dallas"* (1990).

LISA M. BITEL
University of Kansas

GILBERT DAHAN. *Les intellectuels chrétiens et les Juifs au Moyen Âge*. (Patrimoine: Judaïsme.) Paris: Cerf, with the cooperation of Centre National de la Recherche Scientifique, Soeurs de Notre-Dame de Sion, and Fondation du Judaïsme Français. 1990. Pp. 637. 240 fr.

In his pioneering work, *Juifs et Chrétiens dans le monde occidental 430–1096* (1960), Bernhard Blumenkranz traced the role of Jews in European society and the status assigned them from the fall of Rome to the First Crusade. Gilbert Dahan, a disciple of Blumenkranz, has now done the same for the period from the twelfth to the fourteenth century, but his focus is different. Whereas Blumenkranz concentrated on concrete social and legal relations and devoted only about a quarter of his book to Christian thinking about the religious significance of Jews, Dahan concentrates on what the Christian intellectuals of this period knew and thought about Jews and Judaism. The book is divided into five equal parts, but the first two and the last three are so different that they could have been published as separate books, the former for those unfamiliar with medieval Jewish history and the latter for those who work in the field.

The first two parts of the book sketch the persecutions of the period, emphasize the increase in hostility in the thirteenth and fourteenth centuries, discuss the treatment of Jews by secular authorities and the status given them in secular law, and describe church policy toward Jews and their status according to canon law. To cover all this material in 200 pages, Dahan had to be highly selective and is sometimes unreliable. Thus, the medieval concept of the Peace is said to be of feudal origin (p. 78), and it is implied (pp. 152–54) that the popes condemned the accusation that Jews ritually crucified Christian children. Although little in these two parts will seem novel to scholars working in the area, they provide a background for the detailed scholarship in the remainder of the book, which deals with the subject indicated by the book's title and on which Dahan has previously done much original research.

The third part of the book is an analysis of what Christian intellectuals knew about real Jews, based on an admirably wide range of evidence of different kinds: theology, biblical exegesis, canon law, lexicons and translations, quodlibets, sermons, hymns, and so on. It examines in detail the Christian intellectuals' knowledge of Hebrew, the extent of their personal contact with Jews, and their use of Jewish scholarship in establishing the text of the Bible and in dealing with philosophical and scientific issues. Dahan concludes that there was more amiable contact between Christian and Jewish intellectuals, even in the thirteenth and fourteenth centuries, than is usually thought. The fourth part describes the contrasting, almost schizophrenic, development of the negative theological image of the Jew. With its mine of details, it is one of the best general descriptions thus far of the methods, content, and arguments of the polemics against Jews and Judaism. The fifth part concludes by sketching the principal lineaments of the image of the Jew and the main dogmas about Jews and Judaism that the Christian intellectuals developed to defend their faith.

This is a descriptive history of ideas organized

topically rather than chronologically, not an explanatory study of mentality or of individual intellectuals. Although it is possible by assembling references scattered throughout the book to gain some sense of the attitude of an individual intellectual—for example, the extreme hostility of the canonist Guy Terré—there is no attempt to show how certain attitudes were linked or why various individuals viewed Jews as they did. Dahan's approach is thus very different from that of Heinz Schreckenberg, *Die christlichen Adversus-Judaos-Texte (11.–13. Jh.)* (1988), whom Dahan does not cite. (Four years apparently elapsed between the completion of Dahan's book and its publication.) What Dahan has given us is a valuable and rich description of the ideas that intellectuals combined to form their doctrine about Jews.

GAVIN I. LANGMUIR
Stanford University

SYLVIA SCHEIN. *Fideles Crucis: The Papacy, the West, and the Recovery of the Holy Land, 1274–1314*. New York: Clarendon Press of Oxford University Press. 1991. Pp. x, 310. \$82.00.

Few scholars are aware of how many—and how detailed—are the treatises written between 1274 and 1314 on organizing crusades either to support or (after the fall of Acre in 1291) to recover the Holy Land. Fewer still, even if they have some inkling of the intellectual and strategic ferment, know much about the specifics of the plans: the centrality of a naval economic blockade of Egypt, the strong feeling in favor of *passagia particularia* (limited expeditions with professional soldiers) over the older *passagium generale* that we associate with the famous crusades, and so forth. By bringing these and related issues to the attention of scholars, Sylvia Schein's book serves a useful purpose. But compared to the author's claims and the overall execution of the study, the accomplishment is modest.

First, the author has not been well served by her editors. A full list of errors, incoherent passages, and unfortunate editorial choices would take up several pages, but here is a small sampling: "Face with a series of defeats" for "Faced with a series of defeats" (p. 15); "*amor soli matalis*" for "*amor soli natalis*" (p. 27); "Nuremburg" for "Nuremberg" (p. 37); "Anjoy" for "Anjou" (p. 58). One footnote (p. 170, n. 81) has the subject of a study by R. E. Lerner rendered "Guiard-Cressonessant" instead of "Guiard of Cressonessart." A note on the same page (n. 83) has the name of the editor misspelled ("Boutarie" for "Boutaric"), the title of the volume wrong ("*Novices*" for "*Notices*"), and the French word for library in the same title rendered "*Bibliothèque*."

Incoherent sentences are legion: "Thus not even during the fourteenth century did not appeal of the crusade become confined to the upper classes of society" (p. 18); "All around him Islam was prosper-

ing the flourishing" (p. 125). Accent marks are dropped over and over again, or the wrong ones inserted. The editorial decision to leave large amounts of Old French and Old Aragonese untranslated (see, for example, pp. 252–53) is astonishing.

A fundamental argument of the book is that Philip the Fair of France (1285–1314) was an enthusiastic supporter of the crusade. In fact, all that the author shows is that Philip more or less ignored the crusade until 1305. The few times he or his ministers expressed anything about it, they tied their "support" to papal backing for French projects involving the conquest of Aragon or Constantinople. After 1305 the king promised money but did not come through. He opposed many of the central parts of Pope Clement V's project. His brutal suppression of the Templars, as the author stresses, went a long way to undermine crusading plans that had come to be tied closely to the efforts of the military Orders. In 1313, it is true, the king and many of his entourage and family took the cross. The author calls this a sort of "frenzy" of enthusiasm (p. 255). Perhaps, but the king died soon after, and the frenzied family and entourage went about their own business. "The plan," even the author tells us, "collapsed" (p. 256).

It seems to me that this story justifies what Kenneth Setton (quoting Joseph Strayer) affirmed many years ago, namely, that "1285 [the year of the accession of Philip the Fair] marks . . . the end of the crusade as a regular and reliable instrument of papal policy . . . deprived of the steady support of the French king the pope was in a poor position to combat the rising tide of secularism and indifference." Twice the author quotes this statement; indeed, she more or less brackets her book with it. The first time (p. 8) she calls it "rather careless" and "certainly incorrect." The second time—the last note on the last page of the text (p. 268)—she declaims that it is "not justified." Why? Here is her explanation, which has two parts: "This rather careless statement is certainly incorrect, [she writes]. First of all, the crusade, as it persisted after 1285, was anything but a powerful instrument of papal policy" (p. 8). True, but, then, that is precisely what Setton and Strayer are quoted as saying. How does this render their conclusions careless, incorrect, and unjustified? Or is her statement just another example of poor writing, poorly edited? The second part of her explanation is that "Philip the Fair was far less inclined than his father to support some of the policies of popes like Honorius IV and Boniface VIII but subsequently he lent steady support to Benedict XI and Clement V." Yet, as I indicated above, what she in fact shows is that Philip hemmed and hawed, peevishly disputed Clement's plans, gave no money, and destroyed the Templar Order that was at the center of many projects for the recovery of the Holy Land. Is this "steady support"?

WILLIAM CHESTER JORDAN
Princeton University

PETER PARTNER. *The Pope's Men: The Papal Civil Service in the Renaissance*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xii, 276. \$69.00.

This important study, by a distinguished historian of the Renaissance papacy, examines the organization, procedures, personnel, and outlook of the papal bureaucracy from the restoration of pontifical unity under Martin V in 1417 to the Sack of Rome in 1527. It provides an essential perspective on a critical era of papal history, during which the activities of the papal curia proved so consequential for Italy and for Europe.

Peter Partner begins with a succinct sketch of papal administration, describing the various papal offices, their functions, and their responsibilities, before focusing on two categories of papal officials: the clerks of the apostolic chamber, the chief financial organ of the papacy, and the apostolic secretaries, who prepared the briefs and other papal correspondence. The focus on these two groups of functionaries proves a sensible choice. Their status within the curial hierarchy was similar; each underwent significant changes and assumed increasing responsibilities during the Renaissance period; their numbers are sufficient but not unwieldy for statistical analysis; and their curial careers tended to have considerable longevity, enabling a form of prosopographical study. Partner provides in an appendix brief biographical and bibliographical information on over three hundred chamber clerks and secretaries who served during the period 1417–1527, and in chapter 3 traces the careers of a sample of twenty-two such officials.

Partner's study confirms certain long-standing impressions of the papal curia during the Renaissance period, in particular its increasing Italianization and its increasing venality. But his study also corrects certain misconceptions. For instance, the number of papal officials probably only doubled between the fourteenth and sixteenth centuries, a significant increase but not as great as others have supposed. Moreover, except for the office of secretary, prowess in the humanities did not displace expertise in canon and civil law, which remained, as in the Middle Ages, the principal avenue to appointment and preferment. Significant numbers of humanists were indeed recruited for positions in the papal secretariat (42 percent of the papal secretaries during the period 1417–87 can be described as humanists), but after 1487, when a venal college of secretaries was established, the high cost of purchasing the office made its acquisition by a poor humanist virtually prohibitive. Indeed, the vastly extended system of venal offices meant that during the High Renaissance, papal office holding was dominated by Italian notables, particularly those from the papal states and Tuscany. Investing in a curial office increasingly became a strategy by which privileged Italian families promoted and defended their own patrimony.

Humanism, Partner thus seems to imply, was nei-

ther so pervasive nor so influential a force in Renaissance Rome as other scholars have suggested. Particularly after the 1470s, he argues, curial humanists became cultural conformists, emerging as a new kind of papal courtier. But this underestimates their impact. Curial humanists articulated a vision of renaissance and reform, centered on what they regarded as the historical mission of Rome and the papacy, that shaped much of the cultural outlook of High Renaissance Rome and had significant consequences for papal decisions and actions.

CHARLES L. STINGER
State University of New York,
Buffalo

JEFFREY F. HAMBURGER. *The Rothschild Canticles: Art and Mysticism in Flanders and the Rhineland circa 1300*. (Yale Publications in the History of Art.) New Haven: Yale University Press, with the assistance of the Getty Grant Program. 1990. Pp. xii, 336; 237 plates. \$50.00.

The *Rothschild Canticles* is a beautiful and in many ways unique medieval manuscript. Produced most likely in Flanders around 1300 for use by a cloistered woman, its union of text and images creates a visionary mystical experience. Until 1968, when the library of Edmond de Rothschild was sold in Paris, its private ownership made it largely inaccessible to scholars. Now, however, it is in the Beinecke Rare Book and Manuscript Library at Yale University.

Jeffrey F. Hamburger's book on the *Rothschild Canticles* is the first full-length scholarly study of this important manuscript. His work provides information about every aspect of this codex. His first chapter summarizes the codicological structure and discusses the style of the miniatures as evidence for localization and dating. The second chapter explains the book's somewhat unconventional textual contents, which might be characterized as a florilegium of spiritual messages. Several detailed appendices, including a transcription and edition of the unique mystical text of Part I, a complete codicological analysis, and a listing of its extensive miniatures, also help the reader to understand the complexities of the entire manuscript.

The author's primary goal is "to elucidate the iconography of the miniatures" (p. ix). Accordingly, most of the book focuses on this difficult problem. Because the almost fifty full-page miniatures complement rather than illustrate a text, lack any identifying inscriptions, and are often original visionary images, explaining their meaning and place within the programmatic context of the *Rothschild Canticles* is a formidable task. By grouping the illuminations into chapters on related subjects (the paradisiacal miniatures, the Song of Songs miniatures, the Marian miniatures, the mystical union miniatures, and the Trinitarian miniatures), Hamburger fully investigates the sources for the imagery and offers convincing

interpretations of the visual content. Yet a more complete integration of stylistic analysis and insights from codicological examination would have enhanced an understanding of the miniatures' visionary impact and clarified some of the anomalies in the manuscript's organization.

The concluding chapter addresses the place of the *Rothschild Canticles* in the history of medieval spirituality. While this manuscript appears initially to be "a magnificent aberration" (p. 155), Hamburger correctly points out that much of its perceived uniqueness derives from a modern lack of understanding of the varied characteristics of late-medieval mysticism. In fact, Hamburger's work demonstrates that this manuscript aligns with traditions of mysticism current in the Rhineland and Flanders around the fourteenth century. Its visual sophistication highlights some significant aspects of these spiritual currents, including the importance of the interrelationship between verbal and visual imagery and the contribution of women to the articulation of medieval spirituality. The *Rothschild Canticles* also shows that the Rhine area was a center for the development and diffusion of late-medieval mysticism.

Although many of these issues and other aspects of the *Rothschild Canticles* remain to be explored more fully, Hamburger's book provides an excellent and thorough foundation for understanding this manuscript and its place in the history of medieval spirituality.

KAREN GOULD
Austin, Texas

MARGARET BONNEY. *Lordship and the Urban Community: Durham and Its Overlords, 1250–1540*. New York: Cambridge University Press. 1990. Pp. xiv, 307. \$45.50.

Local legend has it that Durham's site was selected by the seventh-century monk, St. Cuthbert, when his coffin refused to budge after a community of his followers, fleeing Viking invaders around 995, passed through a wooded location called Dunholm, on the River Wear. At this place the bishop and monks of the community eventually founded a large Benedictine monastery and cathedral that housed St. Cuthbert's remains and became one of England's most popular medieval pilgrimage sites. Archaeological evidence suggests that the place was, however, settled before St. Cuthbert's followers arrived there and that military and political factors had more to do with the town's origins. Certainly the town's location atop a narrow, steep-sided peninsula surrounded on three sides by the river helped convince its eleventh-century lords to build a castle and fortifications impressive enough to withstand Scottish attacks from the north.

The natural topography of Durham also limited its physical size and was partially responsible, along with

its fragmented lordship, for the town's confusing pattern of separately administered jurisdictions and "boroughs." The fortified section of the peninsula was controlled by the constable of the castle while the densely populated Bishop's Borough, administered by the powerful bishop, was the most prosperous section and contained the marketplace. St. Giles Borough was a suburban jurisdiction held by the master of Kepier Hospital while the priory, through several of its obedientaries or monastic officeholders, controlled the remaining sections; the monastic sacristan ran Old Borough, the almoner administered an area around St. Mary Magdalen Chapel (actually within St. Giles Borough), and the monastic hostellar held both Elvet Borough and the Barony of Old Elvet.

Unfortunately, the surviving documentation from the Bishop's Borough is poor, so most of Margaret Bonney's book concerns the sections of the town controlled by the priory. Because the priory's extant archive contains mostly account rolls, rentals, and deeds, the bulk of the study concerns the urban landscape of Durham, its medieval buildings, and the tenurial and financial relationship between the priory and its urban tenants. The chapter on trades and occupations is largely anecdotal and suffers from the absence of material from the Bishop's Borough, wherein lay the market and the town's commercial center. More illuminating is the chapter that examines the personnel and business of the various Durham courts, although even here the reader suspects that more could be done to wring the social and economic history of the town from the court rolls which do survive. One wonders also why the author did not analyze further the fund of data on building wages and prices. Also missing is an analysis of the role of the professional clerks and other clerical retainers who resided in Durham because of its position as the administrative center of the bishop's palatinate.

Restricted by poor inland communications, Durham never graduated beyond the status of a small market town, albeit one with important administrative functions. Its medieval buildings, surveyed in some detail by Bonney, show that it was not a very wealthy place, although the priory's greater reliance on local markets in the High Middle Ages, combined with evidence for more diverse occupations at that time, suggest that Durham may have been more economically prosperous in the pre-plague period.

MARYANNE KOWALESKI
Fordham University

JOSÉ MANUEL NIETO SORIA. *Fundamentos ideológicos del poder real en Castilla (siglos XIII–XVI)*. (EUDEMA Universidad, textos de apoyo.) Madrid: EUDEMA. 1988. Pp. 269.

José Manuel Nieto Soria has produced a thought-provoking guide to Castilian ideologies of royal

power during the period from the mid-thirteenth century until 1521. Based on a wide reading of available public acts, theoretical treatises, and literary works, his book constitutes a useful guide of the age's conventional political vocabulary. Its utility is enhanced by a glossary of terms that reproduces selections from many of the cited documents.

Nieto Soria has divided his treatment of the theoretical bases of royal power into a defense, much influenced by Ernst H. Kantorowicz (*The King's Two Bodies: A Study in Medieval Political Theology* [1957]) of the importance of the theological and sacred foundations of Castilian political ideology, an explanation of juridical images employed, and a consideration of selected examples to show the connections between ideological principles and political reality. The first and third sections will produce the most controversy.

In the first section he argues, contrary to the views of Teófilo F. Ruiz ("Unsacred Monarchy: The Kings of Castile in the Late Middle Ages," in *Rites of Power: Symbolism, Ritual, and Politics since the Middle Ages*, ed. S. Wilentz [1985], 109–44) and others, that the late-medieval Castilian monarchy was ideologically supported above all by sacred and theological conceptions despite the lack of specific ceremonies and rituals displaying the divine basis of royal power. Nieto Soria thus places himself squarely on the side of those who oppose Américo Castro's thesis (*The Spaniards: An Introduction to Their History* [1971]) that there was a fundamental cultural cleavage between Spain and Western Europe. In the section of examples he paints a picture, reminiscent of nineteenth-century liberal views, of an ideological conflict pitting an ever more aggressive and absolutist royal government against groups who argued for constitutional limits on monarchical conduct. Unlimited absolutism triumphed over its opponents in the *comunero* rebellion of 1520–21 and found its institutionalization in the Habsburg era. Such a thesis will be questioned both by those who see royal officials and rebels as operating in a common ideological environment and by those who feel it neglects the practical as well as theoretical constitutionalism of the Habsburg period.

While Nieto Soria's organization by images makes the book useful to those who want help in locating the expression of particular political ideas, it undercuts his ability to defend his theses, since concepts have often been torn away from the logical steps of which they were a part or from crucial distinctions. Although he tries on occasion to mention some of the connections, his approach makes it difficult for the reader unfamiliar with the sources cited and the details of Castilian political history to understand either the cognitive structures underlying the use of certain images or the writers in the sense discussed by Quentin Skinner (*The Foundations of Modern Political Thought* [1978]). The third section is not detailed enough to ground the abstract images historically. For example, Nieto Soria sees the use of the phrase *el poderío real absoluto* (the absolute royal power) as a

fundamental expression of the developing tendency toward unlimited absolutism (pp. 121–27). Yet the phrase was drawn from a context of conventional judicial theory and practice that gave it a range of meanings which involved limits; Nieto Soria either fails to deal with the essential distinctions or treats them in other sections. These difficulties, however, are probably unavoidable in a work that provides a valuable preliminary survey of medieval Castilian political ideas.

J. B. OWENS
Idaho State University

MARK D. MEYERSON. *The Muslims of Valencia in the Age of Fernando and Isabel: Between Coexistence and Crusade*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xi, 372. \$45.00.

During the reign of the Catholic Sovereigns, Valencia was home to 30 percent of the unconverted Muslims (Moriscos) under Christian sway. This excellent book by Mark D. Meyerson, drawn from an intensive study of the archives of the Crown of Aragón, the Kingdom of Valencia, and the Municipal Archive of Valencia, examines the period 1479 to 1503 (with an eye on the *Germanía* uprisings of 1519–22). Its focus is on the interactions between the crown and inhabitants of royal and seignorial *morerías*. The patient reader will find a wealth of fascinating details unavailable elsewhere on Arabo-Berber modes of social organization that preserved the Moriscos' culture. The rate of apostasy was extremely low because, as the author points out, conversion would have undermined an entire way of life by, to give two examples, ending marriage between cousins, which solidified clans, or preventing ritualized feuding, which assessed standings of families.

Meyerson finds that the Moriscos' economic position had not been in decline since the thirteenth century, as is so often stated. Instead they took full advantage of new opportunities. Fernando followed a consistent policy of trying to lure the Moriscos from the countryside to new quarters in the cities to tap more of their economic potential. Their leaders, however, preferred the protection and isolation of seignorial villages to the temptations and dangers of city life, despite the lighter taxation.

A major portion of this comprehensive study discusses Fernando's often traditional policy toward these people, "the Royal Treasure." During the Reconquest, Christian rulers had borrowed from Islam its universal scriptural injunction to give shelter to the "People of the Book," but in the less-stable form of treaties negotiated by monarchs with individual communities. Despite tensions and suspicions engendered in the Christian masses by the Granada War and the Ottoman Turkish threat, Moriscos in the Crown of Aragón were neither converted en masse nor expelled.

Fernando respected his ancestors' contractual agreements with the Moriscos so long as Muslims contributed to his coffers, although he promoted a tight communal segregation new to Aragonese tradition and foreign to the greater Islamic world. In this, he went counter to what Meyerson finds to be an increasing bilingualism among the Moriscos and the desire of Christian merchants and craftpersons to expand profitable relations with Muslims in the market and the workplace.

Despite the many virtues of this fine book, it nearly runs aground on the rock where many studies of the *Reyes Católicos* founder. Isabel is described as the religious fanatic (because she is a woman, no doubt) and Fernando the cool pragmatist. Yet did not Isabel declare that God had moved her husband to have her establish the Inquisition? There is an illuminating episode in Totosa that confirms—if more confirmation is necessary—what Machiavelli called Fernando's "pious cruelty." Open-minded officials were allowing Muslims to use a church for prayer on Islamic holy days. The king, who pounced on displays of toleration, angrily called for "the oppression of the disorders of the pestiferous and infernal Mahometan sect" (p. 45). Parties to this violation of the church's sanctity by "diabolic" rites faced enormous fines, enslavement, or execution. So much for Fernando's version of coexistence.

MARVIN LUNENFELD
State University College of New York,
Fredonia

MICHAEL NORTH. *Geldumlauf und Wirtschaftskonjunktur im südlichen Ostseeraum an der Wende zur Neuzeit (1440–1570): Untersuchungen zur Wirtschaftsgeschichte am Beispiel der Grossen Lübecker Münzschatzes, der norddeutschen Münzfunde und der schriftlichen Überlieferung.* (Kieler Historische Studien, number 35.) Sigmaringen, F.R.G.: Jan Thorbecke. 1990. Pp. 276. DM 68.

The statistical study of coin finds has been the key to integrating numismatics into general economic history, and this book, in its questions and methodology, is an excellent example of the state of this interdisciplinary field. Extending recent studies of south Germany, Michael North attempts not only to cover northeastern Germany but also to go beyond the study of monetary circulation and into the study of economic fluctuations to uncover the causes of long-term economic regression and expansion in his area. Thus, the first half of the book is a quantitative study of coinage and money supply, which merges into a last half concerned with population, trade balances, prices, shipping indices, and investment levels.

North's "south Baltic region" is the territory north of the Elbe forming the "Wendish" monetary union, including Schleswig, Holstein, Lauenburg, Mecklenburg, and, above all, Hamburg and Lübeck. He

begins with this union's first issue of large coins in 1432 and ends with the region's integration into the new Imperial monetary system and into the new Atlantic economy centered on the Netherlands, in the 1570s. He studies fifty-three regional finds containing 33,000 coins, plus the large Lübeck find of 1984 containing 24,003, which he treats separately as a "snapshot" of monetary circulation in the 1530s.

The south Baltic region suffered a severe monetary shortage in the late Middle Ages, North argues, and he joins monetarist historians in attacking the neo-Malthusian interpretation of late-medieval economic contraction. For gold, the medium of international trade, the south Baltic depended on its commerce with the Rhineland and Netherlands, regions with which it did not have positive trade balances until the sixteenth century. Hence there was a chronic gold shortage in the fifteenth century, aggravated by coinage policies that favored silver over gold. The region also had serious trade deficits with Livonia-Novgorod, Prussia, and England. This money drain worsened as mint output did not even keep up with normal wear and loss (indicated by the average circulating life of coins in the finds). Figures on urban population and rural settlement desertion indicate that population fell less than the money supply. The result was falling prices and profit margins, expensive credit, and reduced investment.

In the sixteenth century, however, a favorable trade balance with the west developed from Hamburg's export of Baltic foodstuffs and raw materials to the Netherlands. This and a quite modest increase in the volume of money more than made up for the continued deficit in trade with the east. North parts company with the monetarists here, arguing that the real force behind the sixteenth-century expansion was not more bullion, or even debasement, but increasing velocity of circulation (measured by shorter coin life), and, above all, increasing population.

North's estimate of the total money volume in 1533, based on the Lübeck coin find, seems wildly low in light of the production of the Hamburg mint in the 1520s (pp. 106, 112–13). This and some of the unclarity in his figures and his methodological explanations do not, however, affect his basic findings. Even if, given the imperfect evidence, one considers his conclusions only plausible—and they are a bit better than that—North's wide-ranging, well-integrated and argued regional study is must reading for students of European economic history at the dawn of the modern age, both for its substance and its method.

THOMAS FOX
McNeese State University

EDUARD MÜHLE. *Die städtischen Handelszentren der nord-westlichen Rus': Anfänge und frühe Entwicklung altrussischer Städte (bis gegen Ende des 12. Jahrhunderts).* (Quellen und Studien zur Geschichte des östlichen

Europa, number 32.) Stuttgart: Franz Steiner. 1991. Pp. xiv, 371. DM 88.

Undoubtedly this is the most comprehensive and up-to-date account currently available of the rise and early evolution of the urban merchant centers of northwestern Russia. Eduard Mühle summarizes and critically assesses the research—particularly archaeological—conducted through the late 1980s. The book consists of three parts: an introduction, the main body of the investigation, and a conclusion, followed by fourteen maps and an unusually complete list of primary sources (unpublished as well as published) and secondary literature.

In the introduction Mühle discusses the notions of town and the rise of urban communities in early medieval Russia, research trends, and problems of methodology and sources. The bulk of the study offers a detailed discussion of the urban centers Ladoga, Novgorod (and its predecessor, Gorodishche), Pskov (including an excursus on its antecedent, Izborsk), Polock, and Smolensk (as well as the earlier settlement in its vicinity, Gnezdovo). In the final chapter Mühle draws some compelling conclusions in a comparative perspective: on chronology and topographic-functional evolution (in three successive phases), on the factors underlying the emergence of the towns, and on the internal and external relationships of these merchant centers at the end of the twelfth century. By taking into account the written records (including birch-bark documents), archaeological evidence (resorting to dendrochronological and stratigraphic dating), and numismatic finds (shedding light on shifting trade relations), the book offers a detailed yet differentiated picture of the genesis and early development of the medieval town in the Russian northwest. The author's intimate knowledge of the progress and findings of local excavations has enabled him to reinterpret, often innovatively, the history of early settlement by various ethnic groups as well as the economic growth and social changes identifiable in that region linking Europe with parts of Asia.

Random examples of Mühle's new insights and assertions include his demonstration that the site of Ladoga from its inception was at some distance from the shores of Lake Ladoga (notwithstanding a higher water level). Still, his assumption that Constantine Porphyrogenitus's *Nevogardas* refers to Ladoga rather than to Novgorod needs further substantiation (p. 52). As for Novgorod, the argument that Gorodishche was the earlier Varangian settlement and that the designation Novgorod ("new town") therefore signals the shift to the new, initially suburban settlements farther north (with a central citadel established by the mid-eleventh century) certainly merits consideration. Mühle further makes a strong case for the controversial view that the Slavic newcomers comprised both east and west Slavic groups and that the original left-bank settlements of Nerev and Lyudin may well

have included, from the outset, both Finnic and Baltic elements while Slavno (or Kholm; compare Old Norse *Holmgardr*) presumably was first settled by Norsemen, who were soon joined by Slavic arrivals. Concerning Pskov and Smolensk, it is noteworthy that the latter initially was the larger community, which was outnumbered by Pskov only after the period considered. Mühle's estimates of the population figures for Novgorod, a maximum of 2,000 in the tenth century but nearly 15,000 by the late twelfth century, seem well-founded and significant. A minor shortcoming is that the author, in discussing the archaeological finds from Gnezdovo, does not mention the vessel with its much-debated one-word inscription, recently boldly reinterpreted by A. Schenker (*Russian Linguistics* 13 [1989], 207–20).

Except for a few errors in the rendition of scholars' names, the work is virtually free of misprints. Overall, Mühle's book is certainly a major contribution to the history both of medieval Russia and the European city.

HENRIK BIRNBAUM
University of California,
Los Angeles

S. M. KASHTANOV. *Finansy srednevekovoi Rusi* [The Finances of Medieval Rus']. Moscow: Nauka. 1988. Pp. 246. 3r. 80k.

In examining the finances of medieval Rus', S. M. Kashtanov focuses on a crucial aspect of Muscovite development: the revenue available to the emerging Russian state from the fourteenth through the sixteenth centuries. Historians have long recognized the importance of this issue for dealing with such themes as modernization (or lack thereof) in early modern Rus', the degree of exploitation of the peasantry, and the relative poverty, backwardness, and underdeveloped administrative machinery of Muscovy in comparison to western Europe. Conclusions on these matters have reflected in large part different authors' assessment of both the resources available to the early Russian state and the effectiveness of the means used to gain access to them. But, lamentably, there remains fragmentary research on these questions. Thus, Kashtanov's work is a significant addition to the existing body of scholarship on this topic.

Not surprisingly for those familiar with his earlier work, Kashtanov's investigation of Muscovite finances concentrates on the taxation policies and practices of the grand princes and the emerging centralized state, particularly as reflected in immunity charters (*zhalo-vannye gramoty*) and other administrative grants providing for limited tax exemptions and the like. In his view the central problem was the effort of the grand princes and their officials to break through the "immunity isolation" enjoyed by the holders of such privileges and to gain access to revenue from their estates. The first major effort at limiting immunities

began in the late fifteenth century and was coupled with the introduction of new impositions, such as "building" obligations (fortifications, roads, and bridges), increased contributions for the postal/communications network, and additional support for military forces. In the second half of the sixteenth century these commonly required services were commuted to monetary equivalents. A second major effort at restricting immunities occurred in 1551, with the government's "review" of privileges granted earlier and the accompanying revocation of many tax exemptions.

But Muscovite fiscal policies were scarcely unidirectional or consistent. Although the expanded grant of privileges during the "boyar rule" years of Ivan IV's minority was followed by the crackdown of 1551, the situation began to loosen up as early as the mid-1550s. During the remainder of Ivan's reign, tax and judicial immunities were awarded for a variety of political and economic reasons connected with the Livonian War, the reestablishment of the Staritsa appanage, and the administrative restructuring connected with the Oprichnina. Although the government had succeeded in imposing similar tax obligations throughout the Muscovite lands by the 1570s, immunity holders rather than state officials were collecting and forwarding the dues from their estates. Simultaneously, peasants on those estates were gradually removed from the pool of those obligated to build and maintain prisons, serve as local government tax collectors, or take responsibility for local antibribe efforts. Thus, Kashtanov finds, the fiscal policies of the Muscovite state in the sixteenth century failed to impose uniformity. During this transitional period both secular and monastic landholders continued to enjoy varying privileges, depending on differing combinations of local tradition, political connections, and circumstances of the individual case. And, while the state succeeded at least partially in imposing tax obligations on privately held estates, it did so at the expense of granting landholders greater authority over their peasants.

Kashtanov's detailed tracing of the twists and turns in Muscovite taxation policies rests firmly on archival evidence accumulated through years of work on the immunity charters. Specialists will undoubtedly want to debate many of his assessments, particularly those arising from what some will see as his tendency to overemphasize the importance of political considerations as the motivating force behind the grant of immunity charters at the expense of possible social and economic factors; privileges bestowed on such men as the Stroganovs, for example, may have been intended equally, or even primarily, to encourage them to colonize their holdings. His heavy reliance on immunity charters granted to leading monasteries leaves unanswered the question of how representative these documents might be, especially for secular estate holders. A related difficulty is his inclination to read into his evidence support for his view

of the Muscovite political process as revolving around the rivalry between the state and various alternative power centers (the church, the appanage princes, even secular landholders). The counter argument could be made, for example, that private landholders who collected tax payments on their estates were, in effect, acting as agents of the centralized government, extending the state's power and authority into areas not yet within the reach of the small proto-bureaucracy of the time.

As Kashtanov notes in his introduction, the history of finances cannot be studied in isolation from that of the growth of the state or the basic problems of domestic and foreign policy. Unfortunately, such isolation is this book's major weakness. Instead of providing a synthesis that builds on his earlier specialized research, in this volume Kashtanov offers another collection of narrowly detailed studies of various aspects of fiscal policy. Most nonspecialists will feel themselves sinking into a morass of detail punctuated by sporadic references to developments such as "guba reforms" or "town fortification work," whose import and significance may or may not come to mind. One can only hope that Kashtanov, given his qualifications, will provide the broadly based summary interpretation of Moscow's financial status that remains so badly needed.

A. M. KLEIMOLA
University of Nebraska,
Lincoln

MODERN EUROPE

RUDI VISKER. *Michel Foucault: Genealogie als Kritik*. Translated from Dutch by J. LEILICH. (UTB für Wissenschaft, Uni-Taschenbücher, number 1600.) Munich: Wilhelm Fink. 1991. Pp. 200.

Rudi Visker approaches Michel Foucault's work as a coherent corpus, a task made easier—unfortunately—by Foucault's untimely death. Visker is aware of Foucault's own protestations against such closure, but he nevertheless discerns a pattern underlying the apparent shifts of approach and theme. His stated intention is to treat Foucault as a serious philosopher: he bases his interpretation on a close and well-documented reading of Foucault's major works—with the surprising exception of *The Archaeology of Knowledge* (1972)—rather than by dwelling on intellectual or cultural context or on secondary literature, although he is clearly aware of and indebted to both. Visker has a gift for clear exposition, and his book will serve as a good introduction to Foucault for readers of German.

The common pattern is a common ambiguity: Foucault approached but never embraced relativism. His claims that truth and knowledge were historically contingent implied that there was no critical standpoint beyond discourse. Yet he was always criticizing particular discourses and invoking alternative ones.

The pattern was set in *Madness and Civilization* (1965): Foucault sought to undermine psychology's claim to scientific authority by showing that its belief in the objective "givenness" of insanity was historically conditioned. Yet by further claiming that psychology suppressed other features of madness that past ages recognized, Foucault assumed that these features were also "given" in some sense. In *The Order of Things* (1966), this ambiguity was generalized: he presented his archaeology as descriptive yet sought to judge the human sciences as inherently unstable in the light of it. This argument will be familiar to readers of Hubert L. Dreyfus and Paul Rabinow's book on Foucault (*Michel Foucault: Beyond Structuralism and Hermeneutics* [1982]); but whereas they saw his later genealogical works as overcoming this weakness, Visker finds it continued—he deliberately treats the "late" *Discipline and Punish* (1977) in the same chapter as *The Order of Things*. Here the ambiguity appeared between the portrayal of power as productive of knowledge and truth on the one hand and power as suppressing other forms of knowledge and truth on the other. Visker sees Foucault's reliance on quotation marks for concepts he wants to question ("scientificity," "body," "power," and so on) as symptomatic of this common pattern.

One can question whether this critique is fair to the historical Foucault, whether he intended anything beyond particular critiques in particular areas rather than formulating a theory of truth or knowledge per se. Visker answers that Foucault's critiques vacillated between the levels of particularity and generality rather than developing a satisfactory apparatus for dealing with either one. Historians might further object that to understand Foucault's appeal is to acknowledge the persuasiveness of precisely this indeterminacy—in the rhetorical impact of his quoted words and their emotional connotations as well as their cognitive associations. But to understand is not to be seduced, and Visker's book provides a welcome exercise in keeping our critical faculties sharpened.

DAVID LINDENFELD
Louisiana State University

LAWRENCE SONDHAUS. *In the Service of the Emperor: Italians in the Austrian Armed Forces, 1814–1918*. Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1990. Pp. ix, 217. \$28.00.

This relatively brief volume by Lawrence Sondhaus is a healthy corrective to both the anti-Austrian accounts of Italian nationalist historians who have glorified every manifestation of anti-Habsburg revolutionary activity and the hagiographic stories of Austrian patriotic writers who contended that the Italians serving in the Habsburg military were bad soldiers and sailors "whose military record by far was the worst of any of the Habsburg nationalities" (p. vi).

To the contrary, the author maintains, the Italian soldiers and sailors in Habsburg service were as competent and loyal as those of the other nationalities in the empire.

A substantial number of Italian troops in Prince Eugene's army accepted Austria's invitation to join the Habsburg army in 1814. Most of them were sent to garrison duty outside of Italy, usually under the command of non-Italian officers. Field Marshal Joseph Wenzel Radetzky believed that the Lombard-Venetian contingents were good soldiers. After the defeat of the Sardinians and their withdrawal from Lombardy in the summer of 1848, he pardoned the Italian troops who had deserted at the onset of the revolution and invited them to resume duty in their old units. During the 1848–49 revolution, "between one-half and two-thirds of the troops" from Lombardy-Venetia "remained loyal." This "compared favorably to that of Hungarian troops stationed in Hungary in 1848" (p. 42).

Very few of the 70,000 Lombard and Venetian servicemen in the imperial army were entrusted with combat roles in 1859, but those who actually fought did not perform badly. In the Seven Weeks' War in 1866 the large majority of the Italians, most of whom had been assigned to August von Benedek's army in the north, fought with considerable courage. In 1867, when a large number of the Italian officers were declared eligible to join the Italian army, "the majority chose to remain in Habsburg service." Their record, according to Sondhaus, "was not markedly worse than that of those from other parts of the empire" (p. 61).

The Italian navy was a much more important component of the Habsburg defense forces than the Lombard and Venetian soldiers, for the Austrian navy had a thoroughly Italian character. In 1817, 73 percent of the navy officers had Italian names. A sizable number of them joined the Italian national cause; nevertheless, when the revolution of 1848 broke out in Venice more than one-third of the officers remained in Habsburg service and moved the ships under their control to Pola and Trieste. These ships contributed to the fall of Venice in 1849 by blockading it from the sea. Later, Italian officers and seamen played an important part in winning the Battle of Lissa for Austria in 1866—a role that, however, in the author's opinion, has been greatly exaggerated by Italian historians.

After the creation of the Kingdom of Italy, the Italian contingents constituted "the smallest of the ten major nationalities" in the Habsburg army, but they still provided roughly half of the common manpower for the fleet (p. 97). During World War I "tens of thousands of Habsburg Italians dutifully served in the Austro-Hungarian army and navy" (p. 105).

In a study based on appropriate documents from the Kriegsarchiv and the Haus-, Hof- und Staatsarchiv in Vienna, the published memoirs of various Austrian and Italian officers, Austrian regimental

histories, and standard military histories, the majority of which were written by Austrians and only a relatively small number by Italians, the author has dispelled many legends perpetuated by the nationalist school of Italian historiography.

R. JOHN RATH
Rice University
University of Minnesota

KRISTINE BRULAND. *British Technology and European Industrialization: The Norwegian Textile Industry in the Mid-Nineteenth Century*. New York: Cambridge University Press. 1989. Pp. ix, 193. \$39.50.

The aspects of nineteenth-century economic history that have gained the most attention in recent years include national income estimates, capital formation, demographic patterns, and wealth distribution. While the transfer of technology has not been neglected, problems with the availability of source materials, especially for the early and middle decades of the century, have hindered the creation of broad syntheses and conceptual breakthroughs. Kristine Bruland's short book counts among its virtues the fact that it identifies an important topic in this field, exploits neglected caches of primary sources, and offers conclusions that have potentially European-wide if not global significance.

After a survey of the relevant literature, Bruland identifies two stages of early technology transfers. The first involved individual skilled artisans who acted secretly in order to avoid the penalties of mercantilist legislation. In Britain's case, this stage lasted well into the nineteenth century. On this subject, the author probably should have followed David Jeremy, John Harris, and others who have placed more emphasis on families and artisanal groups as agents of technological transfer in the Georgian era. This would have made her next stage seem somewhat less novel and more of a logical progression from what had gone before.

The heart of her book deals with the second stage, in which the export of technology was freed from government constraints. For British textile machinery, this occurred in 1843. Down to 1870 (the terminal date of the study) the main agents for this technology transfer were British machine manufacturing firms. To elucidate the contours of this stage, the author has tapped extensive manuscript holdings in Norway related to British engineering firms and their links to the nascent Norwegian factory textile industry. These documents include firm records, personal papers, insurance policies, tax lists, and censuses.

Her most significant finding is that the Norwegians benefited from state-of-the-art "technological packages" from Britain (p. 4). These packages included not only the machines themselves but also information on comparative efficiencies of competing machinery, ad-

vice on their installation and operation, and the provision of British workers to help set them up and supervise a workforce new to the monotonies of factory discipline. The scope of these activities was not insignificant. Between 1856 and 1870, for example, the Hjula Weavery of Oslo was in contact with at least forty-four British engineering firms and 118 other British businesses. During the same period, it employed a permanent (albeit small) contingent of British workers. Hjula and scores of other Norwegian firms also sent representatives to Britain and employed local British agents to obtain information and negotiate contracts. The amount of technical data flowing from Britain across the North Sea was clearly prodigious.

Bruland is especially insightful in identifying the consequences and significance of these findings. She notes that Norway was benefiting from (and contributing to) the mid-century prosperity of British engineering firms. Yet she sees developments in Norway not as a mere imitation of the British experience but as an extension of British industry and an interaction between the British and Norwegian economies. Finally, Bruland doubts that the Norwegian experience with British engineering firms was unique. She can thus plausibly suggest that "the differentiation of the British economy in the early nineteenth century, which produced the world's first comprehensive capital goods industry, produced at the same time the instrument which made European industrialization a practical reality" (p. 154). While the applicability of her findings to other countries must await further research, her well-documented study of the British contributions to Norwegian industrialization is impressive and compelling.

ROBERT GLEN
University of New Haven

SUSAN COTTS WATKINS. *From Provinces into Nations: Demographic Integration in Western Europe, 1870-1960*. Princeton: Princeton University Press. 1991. Pp. xvii, 235. \$42.50.

Most people think that demographic decisions about marriage or family planning are private choices. In her stimulating study, Susan Cotts Watkins shows that fertility is also related to groups that set standards and expectations, and she argues convincingly that, in Western Europe between 1870 and 1960, the community determining demographic behavior shifted from the local to the national level. In Watkin's view this is to be attributed to the gradual "nationalization" of markets and culture, which went along with a waning of regional differences and the creation of a national community going beyond politics and economics. As local societies were drawn into national networks, demographic solidarity, largely local in 1870, became largely national by 1960. In 1870, "national boundaries were faint, regional boundaries

vivid" (p. 3). In 1960, the situation was reversed, nation-building extended to bed and hayloft.

Between 1870 and 1960, marital fertility fell, the proportion of married people rose, illegitimacy and infant mortality declined: as national consensus about family size spread, class differentials in fertility decreased. The use of contraception is a private act, but one on which society has an influence. Evidently, national political processes are mirrored in private as in public behavior.

Watkins's work covers fifteen countries over one century. I wish she had told us more about natalist policies and family allowances. But specialists can benefit from her detailed tables; innocents can benefit even more from an argument so clear that even I can understand it. Lucid and compelling, this is a useful book. I must report, with no complaint, that it tends to privilege France, on which the author focuses one of her most original chapters, titled "From Peasants into Frenchwomen," which follows immediately on a chapter that finds economic homogenization less statistically significant than linguistic integration. Decline in linguistic diversity, says Watkins, goes with decline in demographic diversity.

This makes sense. A common language is the basis for effective interaction. Accent and dialect reflect and reinforce differing concepts, values, and rules of behavior. No wonder Watkins finds a distinct language associated with distinctive fertility, whether unusually high or low. Distinct languages, markers of distinct cultures, inhibit broader communication, prohibit larger community. As community expands from local to national (Watkins notes the power of television to cross linguistic as well as social barriers), the circumstances of life become more alike within each country; people increasingly speak the same language, eat the same foods, buy the same toys, and follow the same fashions in fertility as in other things.

None of this, as the author warns, works tidily. Linguistic history continues to matter even after linguistic homogenization; cultural residues survive in demographic behavior (she has interesting things to say about how persistently some regions defy dominant trends), but national demographic integration, especially after World War I, cannot be gainsaid.

Having presented her conclusions, Watkins risks the prediction that, in the years to come, as economic boundaries coincide less with national ones, national boundaries will once again become less deeply etched on the demographic map of Europe. In other words, the acculturating process that she has traced so well will be played out further, this time at the European level where economic and political integration foreshadows greater demographic similarities. Yet "national definitions will persist as shadows of tone if not of color on the demographic map of Europe" (p. 80).

EUGEN WEBER
University of California,
Los Angeles

ELAINE SHOWALTER. *Sexual Anarchy: Gender and Culture at the Fin de Siècle*. New York: Viking, 1990. Pp. xii, 242. \$19.95.

The Pompidou Center's giant red and blue cylinders challenge the soft grays of medieval and Renaissance Paris. Hanging high above the Pompidou's courtyard, a giant electric clock, the Génitron, flashes out the twentieth century's remaining seconds. With this image, Elaine Showalter opens her fascinating tour de force, reminding us at the same time of Angela Carter's biting remark, "the *fin* is coming a little early this *siècle*."

Showalter's book, she tells us, is "about the myths, metaphors and images of sexual crises and apocalypse that marked both the late nineteenth century and our own *fin de siècle*, and its representations in English and American literature, art and film." "In periods of cultural insecurity," Showalter continues, "when there are fears of regression and degeneration, the longing for strict border controls around the definition of gender, as well as race, class and nationality, becomes especially intense" (pp. 3, 4). Evoking Mary Douglas's brilliant contentions that the body politic inscribes those physical bodies subject to it, Showalter argues that both the Victorian and contemporary British and American middle classes sought (and seek) to allay their fears of radical and uncontrollable social change by regulating the bodies of those who appear most threatening and, ironically, most controllable. The parallels between both *fin de siècle* is striking.

Showalter's study focuses on two bodies, middle-class white women and gay men. Citing Carol Pate-man, Showalter contends that, on the one hand, women exist at all times on the boundaries of symbolic order. Constituting a frontier between men and chaos, they inhabit a mysterious and frightening wild zone outside patriarchal culture. Gay men, on the other hand, render men's gender identity as unstable as women's, making all gender categories problematic.

Showalter builds her book around an analysis of female and gay mythic figures and stereotypes, figures that first appeared in Victorian England, figures that, alarmingly, have reappeared, cloaked in their initial intensity, in the late twentieth century. The work counterposes the Odd Woman and the Odd Man, the New Woman and the Romantic King, the Veiled Woman and Mr. Hyde. Each chapter is organized around a series of intense dialogues between the feminists and the misogynous men of the late nineteenth and late twentieth centuries. As a result, gender and sexuality are presented as highly contested phenomena and meaning as ever-changing, fluid and dynamic.

"Sexual anarchy," Showalter tells us, "began with the odd woman. The odd woman—the woman who cannot marry—undermined the comfortable boundary system of Victorian sexuality and gender roles"

(p. 19). Men represented her as hysterical, sexually starved, pathetic. They feared her as representing a social and sexual group that justified women's suffrage and as an economic rival for middle-class jobs. Feminists seized the odd woman as their own, rechristened her the "new woman," and praised her social and sexual autonomy. She proved, they claimed, that Victorian gender polarities and marriage structures were anachronistic and oppressive. Lying behind the debate about the odd/new woman were fears of female and male homosexuality. For every odd woman there was an odd man. Same-sex bonding and eroticism characterized women's colleges and men's clubs alike. Indeed, the odd man and odd woman shadowed one another through every Victorian myth of sexual disorder and anarchy.

Focusing increasingly on literary and medical texts, Showalter systematically emphasizes the gay male figure who constitutes the subtext of so much of male *fin de siècle* literature. In her fascinating readings, Dr. Jekyll's Mr. Hyde emerges both as a gay young tough and the homosexual secret of Victorian male "clubland"; Salome is unveiled as Oscar Wilde desiring John the Baptist; Dracula, living on the borders of three states (masculinity, femininity, and bisexuality, Showalter suggests), desires men as well as women. Showalter ends her book with a fascinating three-way comparison of gay male politics in Victorian England and those same politics before and after the emergence of AIDS.

This is a book about crossings—the crossing from one century to another, between the body politic and the physical body, between literary and social history, myth and social practice, genders and sexualities, order and disorder. There are issues I would have liked Showalter to include. For example, I would have liked her to expand her examination of the new woman beyond the actively heterosexual to those women reformers and writers whose expressions of female eroticism I feel problematize twentieth-century understandings of sexuality; to extend her focus beyond the male quest for romance to examine Hardy and the American Naturalists and social realists; and to more equitably weigh her British comparisons with Americans by enlarging on her analyses of America in both centuries. Yet, as Jean Valentine once told a poetry class, "That is [my] poem. It is not her poem." Showalter's book is fascinating. Treading new ground, opening new questions, it is like the texts it analyses: a bit scandalous, certainly flamboyant, suggestive, and fun. This is a book to include in any history course on the late nineteenth century.

CARROLL SMITH-ROSENBERG
University of Pennsylvania

SOPHIE DUBNOV-ERLICH. *The Life and Work of S. M. Dubnov: Diaspora Nationalism and Jewish History*. Translated by JUDITH VOWLES. Edited by JEFFREY SHANDLER. (Modern Jewish Experience.) Bloomington:

Indiana University Press, in association with the YIVO Institute for Jewish Research. 1991. Pp. x, 284. \$29.95.

Published originally in Russian some forty years ago, Sophie Dubnov-Erlach's memoir of her father, the Jewish historian and nationalist ideologue Simon Dubnov, provides a welcome and unusual glimpse of the private side of one of East European Jewry's most influential public figures. What is best known about Russian Jewish life in the late nineteenth and early twentieth centuries—the period when Dubnov was at his most productive as both a historian and political theorist—are its competing ideologies, especially Zionism and Jewish socialism, born out of the mounting Judeophobia of late-imperial Russia. While his daughter's memoir conveniently (if also somewhat perfunctorily) summarizes Dubnov's many books and articles, its main contribution is in its portrait of his life as drawn mostly from his own three-volume autobiography *Kniga zhizni* (The Book of My Life), which remains unavailable in English. In its vivid portrayal of the mundane tribulations and joys of his daily life, his daughter's account is superior even to Dubnov's: it is clearly the best available source for the English reader interested in the day-to-day routine of a member of the Russian Jewish intelligentsia.

Dubnov is best known for his ten-volume history of the Jews, still a standard text, in which, inspired by evolutionist, liberal nationalist, and utilitarian assumptions, he charted how the Jews had persevered as a nation. Communal self-government stood out in his mind as especially important in this regard perhaps both because of its apparent resemblance to the Russian peasant commune and because it permitted him to use a Rankean statecraft-oriented model for the reconstruction of Jewish history. Communal autonomy, he believed, represented a compromise between national independence (he eschewed Zionism as illusory) and submergence in an assimilatory *Rechtsstaat*. In contrast to many of those closest to him (such as the Jewish cultural nationalist Ahad Ha'am, for instance), Dubnov was less distrustful of the larger world, more willing to accept partial solutions, and less willing to believe that problems were amenable to thoroughgoing change.

A political moderate by the rarified standards of his highly charged milieu and a historian who distrusted all manifestations of radicalism (they also fascinated him; hence his pioneering social history of Hasidism), in his private life he was the severest of men. An ascetic whose only luxury was his daily walk, he was capable of sustaining a regimen of sixteen-hour workdays that resulted in periodic blindness, and he took for granted that the needs of his family, especially those of his strong-willed but devoted wife Ida, be subordinate to his well-honed scholarly agenda. A secularist in his young adulthood despite (or, because of) a profoundly traditional upbringing, he refused at first to marry Ida in a religious ceremony. (She

agreed to live with him out of wedlock.) His daughter appears ambivalent about his self-absorption: sympathetic to what she sees as the demands of genius but resentful of his amiable but relentless distance from household affairs.

This was an extremely poor household until late in Dubnov's life, when his textbooks and teaching began to supplement what was until then a family supported almost entirely by Dubnov's journalism, which, in turn, he resented as a diversion from his scholarship. The memoir provides vivid descriptions of the flats—often damp, suburban, and precariously furnished—in which Dubnov charted the past and future of the Jewish people. The family's residence in St. Petersburg was for many years illegal; during their years in Odessa, between 1890 and 1903, the most fertile literary period of Dubnov's life, they were almost always poor. His ideological masterpiece, *Letters on Old and New Judaism* (1907), his monograph *The Inner Life of the Jews in Poland and Lithuania in the Sixteenth Century* (1900), and much else were written under extraordinarily trying circumstances.

Dubnov-Erich has a fine eye for the discreet drama of domestic life; she also describes without the standard pieties those around Dubnov, among them the Yiddish writer Mendele Mochor Seforim and Ahad Ha'am who came to achieve an almost fabled status in the Jewish collective memory. Writing as a lifelong devotee of the Jewish Socialist Labor Bund, Dubnov-Erich particularly dislikes Ahad Ha'am—indeed, she appears perplexed by her father's close friendship with him—whose disavowal of political activity outside the Jewish national arena she sees as indicative of “a peculiar entropy of civic feeling” (p. 137). Whether or not her political judgments withstand the test of time, her portraits of Dubnov's circle are helpful for any student of the period eager to visualize the central figures in Russian Jewish intellectual life as more than mere ideological mouthpieces. She describes Mendele's sitting room furniture, the way in which Ben Ami (a well-known Jewish nationalist whom she calls a “mediocre Russian Jewish fiction writer”) carried himself, and the discussions of stocks and bonds between the financially naïve Dubnov and the Yiddish writer (and keen investor) Shalom Aleichem.

The translation is fluent and a glossary and editor's notes render the volume accessible to non-specialists. An introduction by Jonathan Frankel provides a superb and probing analysis of the interplay between politics and scholarship in Dubnov's long career; he died at the hands of Nazis in the Riga ghetto in 1941 at the age of eighty-one. The afterword by the Russian literary historian Victor Erlich, Dubnov's grandson and the author's son, is a deeply affectionate portrait of Dubnov in old age written, perhaps unsurprisingly, without the ambivalence that permeates his mother's account.

STEVEN J. ZIPPERSTEIN
Stanford University

DEBÓRAH DWORK. *Children With a Star: Jewish Youth in Nazi Europe*. New Haven: Yale University Press. 1991. Pp. xlvii, 354. \$25.00.

The publication of a myriad of memoirs on the Holocaust easily lulls one into believing that all has been told about the history of this event. But this is not the case, as Debórah Dwork demonstrates. Only 10 percent of the Jewish children in Europe in 1939 survived. The vast majority of those sent to death camps were killed immediately. Children succumbed to disease and starvation. But these are not the only reasons their story has not been fully told. In the immediate aftermath of the war few people—adult survivors included—were willing to pay attention to the children's stories. Some did not consider them articulate witnesses. Many historians assumed that the child's experiences were no different from the adult's: hunger, brutality, hard work, and incredible pain and loss. Yet other adult survivors, including the leaders of the major survivor organizations, dismissed the children's stories, particularly those of children who were hidden, as inconsequential in comparison to the stories of those who had survived the death camps. Dwork demonstrates how wrong these views were.

It is the story of the children in hiding that is most compelling. Parents had to decide to relinquish their children, often to utter strangers. The first step in the chain of rescue was recognition by parents that they could no longer protect the very being to whom they had given life. Irrespective of whether the person to whom one entrusted the children was an acquaintance or a stranger, “the initial act of abdication was the fundamental beginning” of rescue (p. 65).

The other major players in this story were the rescuers, many of whom were women. They could pass through German-occupied territories with children in tow without arousing suspicions. Sometimes they were part of a larger network, but in many cases they operated on their own. Their story has not been told for a variety of reasons. The topic of Christian rescuers has only begun to be investigated in recent years. It took a number of decades for the children who were saved to come of age and call for the recognition of their rescuers. More importantly, “unlike the men who had joined the official resistance . . . these women to this day speak unceremoniously of their activities as just another job that had to be done and they insist they are not remarkable for having undertaken it” (p. 41).

The children banished their own culture to the most distant recesses of their memories. Successful rescue meant suppressing who you really were. Parents and children genuinely feared that if the children were too successful at this they would not remember who they were when the war ended. Some children found loving homes while others became “Cinderellas,” forced to do hard and menial labor. But even in those homes where children were treated

poorly the people who were hiding them risked their lives. The children lived in a trap of gratitude and self-abnegation, living with people to whom they felt obliged to please and toward whom they were beholden because these gentile adults—regardless of how they treated them—risked their own lives in order to allow them to live. The children had to confront the question of the value of their Jewish identity. Many recall having “felt ashamed” about it (p. 104).

Children in other settings, including transit camps, ghettos, and death camps, also faced unique problems. In certain cases they had to make decisions for their parents. One fifteen-year-old girl told her mother, who was considering accompanying her on a transport train from Theresienstadt, “No, in this time you do not do anything out of your own free will. You stay here. I go by myself.” Then, she recalled, “I went on the train” (p. 153). Children in ghettos often became responsible for smuggling food for their entire family. Parents lost their authority over their children. They no longer had the ability to protect them and often could not provide the emotional support they needed.

Although many of the details Dwork relates are well known, telling it through the eyes of the children helps bring into sharp focus the injustice and horror that has become routinely known as the Holocaust. It is a compelling and well-researched account that rescues an important part of this story from oblivion. Many of her informants had never before told their story. The past had become an unspoken part of their memories, “an unopened internal package” (p. 270). When Dwork asked the one hundred respondents why they were willing to share their memories they all had a uniform response, “because you asked” (p. 270). It reminds us that even when we think we have heard it all, we too must continue to ask.

DEBORAH E. LIPSTADT
Occidental College

KENNETH C. BARNES. *Nazism, Liberalism, and Christianity: Protestant Social Thought in Germany and Great Britain, 1925–1937*. Lexington: University Press of Kentucky. 1991. Pp. 203. \$26.00.

In this book Kenneth C. Barnes explores an observation at once commonplace and largely unexamined: whereas Protestants in Germany were proverbially amenable to authoritarian nationalism in general and National Socialism in particular after 1918, their counterparts in Britain and elsewhere typically responded quite differently to the sundry crises of the interwar period. Drawing on the extensive files of the Life and Work movement—arguably the preeminent forum for international Protestant dialogue between 1925 and 1937—Barnes contrasts pronouncements by British and German church leaders on such topics as unemployment, class conflict, and the church's role

in society. The aim of his study is to show “how different mentalities operated to find meaning in the interwar situations and directed people toward appropriate social and political action” (p. 134).

These differences of mentality, Barnes argues, were fundamentally theological. For their part, most British church leaders embraced a liberal social gospel marked by optimistic views of human nature, immanentistic conceptions of the kingdom of God, and considerable confidence in the church's capacity to promote social and economic reform. In Germany, by contrast, where liberal theology was crippled by institutional isolation and the shock of World War I, Protestant social conceptions were more likely to reflect pessimistic traditions of thought that stressed human sinfulness and effectively denied the church any mandate to address the concrete problems of society. Broadly speaking, the British social gospel helped buttress a reformist consensus that anticipated many elements of the postwar welfare state; the German version served primarily to undermine alternatives to the fascist project.

Barnes has provided a thoughtful analysis of the role of theological reflection in the formation of political culture. His focus on social thought yields fresh and sometimes provocative insights, as for example in his assessment of Barthianism, which Barnes sees less as a new breakthrough in German theological consciousness than as yet another alternative formulation of the regnant pessimistic consensus. Although he eschews comprehensive explanatory formulas, Barnes appears to posit a basic affinity between theological and social liberalism. Terms such as “liberal” and “conservative” are of course notoriously malleable, and at some points their analytical force is not as clear in Barnes's presentation as one might wish. Thus, Barnes describes Jakob Schoell, chairman of the influential social committee of the German Protestant church federation, as a “conservative clergyman” affiliated with the “ultraconservative *Christlich-Sozialer Volksdienst*” (p. 73). Yet Schoell was in fact a Ritschlian liberal in theological outlook as well as a veteran Naumannite social activist; he was also a *Vernunftrepublikaner* whose support for the CSVD after 1930 probably had less to do with the Stoeckerite pedigree Barnes adduces than with the relatively populist image of the party in Schoell's native Württemberg.

Here and elsewhere Barnes's categories invite further refinement. He has, however, marked out a promising line of inquiry, one that certainly deserves further exploration.

DAVID J. DIEPHOUSE
Calvin College

JANET M. MANSON. *Diplomatic Ramifications of Unrestricted Submarine Warfare, 1939–1941*. (Contributions in Military Studies, number 104.) New York: Greenwood. 1990. Pp. xvi, 215. \$39.95.

Behind the awkward and misleading title of this slender volume lurks what is clearly—and sometimes painfully—a mildly reworked doctoral dissertation. Its core consists of Janet M. Manson's diligent researches into naval archives and related materials in both Germany and the United States, and her findings are well worth reporting in a journal article, or perhaps even two.

Manson fleshes out previous accounts of German submarine policy between 1939 and 1941 and strengthens the case of those who have already argued that Adolf Hitler consciously and deliberately avoided both the term and the full practice of World War I-style unrestricted submarine warfare, and restrained his admirals until December 9, 1941, largely in the interest of keeping the United States formally out of the war. She even provides a plausible explanation of why Hitler nonetheless declared war on the United States almost immediately after the Pearl Harbor attack. Manson also traces U.S. policy relating to submarines over the same time span, sheds some additional light on the *Greer*, *Kearny*, and *Reuben James* incidents (complete with naval charts), and provides the fullest explanation yet of the implementation, on December 7, 1941, of America's "unrestricted submarine warfare" against Japan.

Beyond this, however, the volume really has little to say. The opening section on World War I adds nothing to earlier accounts except a bit of confusion; the interwar period seems largely irrelevant to the whole subject, and a chapter on "American Neutrality, 1935–40" is little more than a diffuse summary with a few errors thrown in. It is surely not helpful to claim that the "cash-and-carry" provision of the 1937 Neutrality Act allowed belligerents to purchase arms from the United States (p. 55); that this provision was "designed to aid France and Britain in the event of a European war" (p. 56); or that Germany's war aims appeared "boundless," even in 1938 (p. 59). Nor does it inspire confidence to learn later that Hitler served as minister of war during World War II when, in fact, he had abolished the War Ministry in 1938 (p. 162).

The American espousal of more or less unrestricted submarine warfare in 1941 (which Great Britain had done somewhat earlier) produced problems at the Nuremberg trials, where German admirals Erich Raeder and Karl Doenitz were charged with conducting unrestricted submarine warfare as a criminal act. A number of American admirals, including Chester Nimitz, supported the defense, and the Germans were not convicted, apparently on the grounds that what they had done had also been done by their opponents in the war. That is an interesting point, but not a great deal can be made of it. Moreover, not even by the widest stretch of the imagination can the American decision to use submarines against Japan be regarded as the greatest foreign policy reversal in American history, as Manson claims (p. 1). In the final analysis, therefore, her actual

subject is a very modest one, which does not really justify a monograph.

MANFRED JONAS
Union College

GLYN REDWORTH. *In Defence of the Church Catholic: The Life of Stephen Gardiner*. Cambridge, Mass.: Basil Blackwell. 1990. Pp. xii, 354. \$49.95.

Stephen Gardiner was a major figure in English politics and religion during the reign of Henry VIII. As Bishop of Winchester he was one of the wealthiest and most influential churchmen. Although he acquiesced in Henry VIII's break with Rome, he never abandoned his traditional religious views; during the 1540s he was a leader of the conservative faction, opposing Thomas Cranmer's reforms in the church and Thomas Cromwell's policies of state. Imprisoned by Edward VI, he reemerged to become chancellor under Mary Tudor. He helped Mary restore papal jurisdiction but died halfway through her reign.

We have long needed a new life of Gardiner, and Glyn Redworth's study is especially welcome since it is based on extensive new research and presents a number of new interpretations. Unlike most biographers, Redworth finds his subject less influential than we had thought. He denies that Gardiner played a major role in Cromwell's fall, attributing it instead to the king's "lust" for Catherine Howard (p. 119). He argues, too, against the view that Gardiner's opinions were influential in shaping the *King's Book*, a conservative theological compendium published in 1543, or the act limiting Bible reading passed by Parliament in the same year. Henry himself was more concerned with these matters.

Rather than seeing Gardiner and the Duke of Norfolk as joint leaders of a conservative faction, Redworth emphasizes their differences over foreign policy: Gardiner consistently advocated an imperial alliance, while Norfolk remained stubbornly pro-French. Although Gardiner was naively optimistic about the possibility of a negotiated settlement between Henry VIII and the papacy, especially at the Diet of Regensburg in 1541, his personal belief in papal supremacy did not revive until his years in the Tower. Despite his intelligence and position, Gardiner was neither a saint nor a true leader. He suffered from cowardice, was tactless, lost his head in a crisis, had few friends, and was always blinkered by his devotion to the old religion.

Redworth's account should be seen as part of the recent attempt to write the Catholic history of the English Reformation. In company with J. J. Scarisbrick (*The Reformation and the English People* [1984]) and Christopher Haigh (*The English Reformation Revised* [1987]), he emphasizes the continuing appeal of traditional religion rather than the enticement of Lutheran reform. Such an attitude is more helpful in understanding Gardiner than it has proven to be in

some other contexts; even those whose personal loyalties lie elsewhere can accept it as a valuable corrective to the viewpoint of writers like A. G. Dickens (*The English Reformation*, 2d ed. [1989]). The weaknesses of Redworth's book lie more in its excessive tendency to speculate about Gardiner's early life and its disappointing brevity on the reign of Mary, when Gardiner finally attained high office, than in its doctrinal assumptions.

STANFORD E. LEHMBERG
University of Minnesota,
Minneapolis

LINDA LEVY PECK. *Court Patronage and Corruption in Early Stuart England*. Boston: Unwin Hyman; distributed by HarperCollins Academic, New York. 1990. Pp. xii, 319. \$59.95.

Conventional wisdom has it that corruption flourished in early Stuart England, that as time went on the disgust of "the country" at the laxity of "the court" increased, and that the resulting decline in the prestige of the monarchy contributed to its collapse in the 1640s. Linda Levy Peck, whose previous book on the earl of Northampton dealt with some aspects of this question and its connection with patronage (*Northampton, Patronage and Policy at the Court of James I* [1982]), has now given us a large and very detailed study of the period between 1603 and 1640 (Northampton died in 1614). She concludes that conventional wisdom is correct, although the problem is immensely complicated, and it is not at all clear whether there was any solution to the Crown's interconnected problems of revenue shortfall, debt, inflation, and an inadequate tax base.

What constituted corrupt practice? The line was difficult to draw. Paying fees to get your court case expedited was allowed; bribing a courtier to get a government contract was not. Purchase of an office from the previous holder was accepted practice, though legally questionable; purchase of a title of honor was denounced as "temporal simony." The oath taken by the controller of the navy illustrates the difficulties well; he swore to execute his office in "no way particular to [his] own profit by his Majesty's loss" (p. 108). In other words he could make as much profit as possible, as long as the king's pocketbook did not suffer. The career of Sir Robert Mansell is a wonderful example of the paradox of Jacobean and Caroline government. He made a considerable fortune as treasurer of the navy between 1604 and 1618 by thoroughly corrupt means: the creation and sale of unnecessary offices, kickbacks from suppliers, the diversion of naval stores into private hands. His crookedness was no secret, but he had a powerful protector in Lord Admiral Nottingham and was allowed to sell his office when Buckingham took over the admiralty. In his retirement he secured a patent for a new process for making glass. The business was

immensely successful, producing better glass at a cheaper price, especially for windows and bottles. Corrupt practices could redound to the public benefit.

In common with other analysts of the causes of the Civil War, Peck identifies the 1620s as a crucial turning point. The definition of corruption was greatly expanded to include gifts as well as bribes—hence Bacon's fall—and the rhetoric of corruption was used to attack not only unpopular monopolists like Mompeyson and unwanted officials like Cranfield but also the great royal favorite Buckingham, whose real offenses were his unpopular policies and his monopolistic direction of the golden stream emanating from the Crown. Because of Buckingham, royal patronage itself became associated with corruption; it was, says Peck, "neither the tie nor the salve it was intended to be" (p. 211).

Peck spells out her argument in a series of carefully argued topical chapters dense with detail that sometimes becomes a bit wearying, especially in the long, discursive footnotes. Factual slips are few and minor. This is an important, learned, and valuable book that every student of the period should read.

MAURICE LEE, JR.
Rutgers University,
New Brunswick

J. V. BECKETT. *A History of Laxton: England's Last Open-Field Village*. New York: Basil Blackwell. 1989. Pp. xiv, 340. \$60.00.

Laxton is the only village in England in which the full system of open-field farming has survived into the twentieth century. J. V. Beckett, an exceptionally skilled and thorough agrarian historian, has produced an authoritative history of this uniquely important community. The late survival of its open fields has produced unusual written and oral documentation, permitting an unparalleled insight into practices that may be a thousand years old. This book is certain to be much cited in the literature.

In some respects the book is disappointing. Despite the claim on the dust jacket that this is a history of the village "over the last millennium," Beckett is able to tell us little about its early history. We learn demographic trends, manorial descent, and minor information on such matters as clearance, but Laxton teaches us nothing about the early evolution and chronology of open fields, early customs of the manor, or even the sentiments and role of such a village during the English Civil War. Silence on these matters is not the fault of Beckett. Good records for this well-documented village only begin in 1635. But we are left wondering whether this remarkably full view of open-field farming is entirely applicable to conditions prior to the seventeenth century.

Laxton, despite the fame of its open fields, is not typologically representative of Midlands arable-dom-

inant villages. It does not lie fully within Joan Thirsk's lowland classification. This is a forest village (the medieval administrative center of Sherwood Forest), lying on rather heavy Keuper Marl soils that were receptive to arable cultivation but not highly suitable to it. Much of the village's early history is more similar to forest and wood pasture regions than to prime arable land. Only a small proportion of land was cleared and under cultivation in 1086. Clearance was slow and prolonged. Early nucleation was accompanied by significant numbers of dispersed farmsteads and one hamlet (the latter persisting into modern times). Such patterns contribute to my unease over projecting into the medieval centuries the rich details of arable open-field farming that we learn from records from the seventeenth century and later.

Beckett has a rich story to tell of the seventeenth century, especially concerning the land market. Contrary to all previous investigations, this book shows that small freeholders in Laxton dramatically increased in numbers between the surveys of 1635 and 1736. They then further increased, as earlier land-tax historians found, until 1815. They experienced hard times and some contraction afterward, but their numbers only declined catastrophically during the agricultural depression at the end of the nineteenth century. This picture is all the more convincing because it depends essentially on a rich and full sequence of surveys and not on tax documents, which have traditionally been employed but are wholly unreliable on this question.

Beckett also has important new insights into factors impeding enclosure. He suggests that small freeholders increasingly farmed tenanted acres toward the end of the eighteenth century, and these newly complex patterns of holdings discouraged or reversed the processes of consolidation and engrossment that normally facilitated enclosure. The book gives unusual attention to farm tenancy, which is one of the least understood aspects of farming in this period. Tenancy was of far greater economic significance than small freeholding, which has traditionally and unfortunately been the focus of specialists' attention. Beckett is wrong to be surprised by the patterns of leaseholding he notes among freeholders. They were widespread. They may or may not have retarded enclosure, but he is right to note their general importance.

DONALD E. GINTER
Concordia University

HENRY ABELove. *The Evangelist of Desire: John Wesley and the Methodists*. Stanford: Stanford University Press. 1990. Pp. xii, 136. \$25.00.

The fact that followers of the Reverend John Wesley lined the roadside and curtsied as the evangelist passed by does not strike historians as extraordinary. After reading Henry Abelow's ingenious analysis of

the movement and its leader, however, this modest gesture takes on a multiplicity of meanings worth pondering. Deference, Abelow argues, was a crucial part of Wesley's carefully orchestrated appeal as an evangelist. By consistently playing the gentleman, he was able to woo followers into a relationship demanding submissiveness and respect. Abelow carves a portrait of a highly egoistic man dead set on creating a movement all his own.

Yet this is only one side of the account. By examining personal documents of preachers and other people within the fold, materials preserved for posterity with rather different expectations than those borne out by this book, Abelow unearths a complex variety of responses among Wesley's followers. Curtseying did not rule out defiance. Children should not play, Wesley intoned, but mothers ignored him, responding with the apparently perennial dismissal: "Oh, he has no children of his own!" (p. 102). One such example might seem inconsequential, but Abelow, who has an eye for contradictions and the fetishes that help to create them, provides a cornucopia of revealing tidbits. Sparing lengthy explanation and excuses, he convinces us that his arguments make good sense.

Abelow contends that this somewhat peculiar dialectic between leader and followers gave Wesleyanism a unique longevity among eighteenth-century evangelical movements. Challenging traditional hagiographic accounts, which have attributed nearly everything to Wesley's powers as a religious leader, Abelow systematically dissects and disempowers the great father. Wesley provided everything from electric shocks for the ailing to pirated editions of literature in order to circumscribe the world his followers inhabited. But all (or almost all) was for naught.

In matters of sexuality, for example, Wesley tried but failed to control the activities of his followers, and repeatedly his best-laid plans for spiritual intimacy running vertically between flock and shepherd resulted in a veritable explosion along horizontal lines. Followers, not Wesley, initiated some of the best features of Methodist organization, Abelow points out in an important section of the book (pp. 45-48). Here Abelow would have done well to acknowledge Wesley's ability to respond to followers' initiatives. However much the egoist, Wesley often capitulated to pressure from below; this, in particular, lent his movement uniqueness.

Abelow's book brings John Wesley and the Wesleys down to earth. One might even read it not as a study of evangelicalism, but rather as demystification of the historical subject that makes the eighteenth century feel a great deal closer than ever before. Historians of Wesleyanism will not be particularly pleased with Abelow's iconoclasm, and historians of religion may blanch at the summary treatment of various aspects of doctrine and practice, such as charity and discipline. This is, nevertheless, a fascinating and often beguiling look into a religious move-

ment. To paraphrase an oft-quoted assessment of psychoanalysis, the book will not change one's mind about Wesleyanism, but one's perception will never again be the same.

DEBORAH VALENZE
Barnard College

SARAH PALMER. *Politics, Shipping, and the Repeal of the Navigation Laws*. New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. ix, 209. \$59.95.

The repeal of the Navigation Laws in 1849 ended two centuries of legislative protection for British shipping. Few will deny the importance of these laws over time, yet surprisingly Sarah Palmer's study is the first to treat the subject of repeal, widely defined, in a book-length work. In part this is explained by the greater popular and historical attention paid to the Corn Laws, and in part by the immense complexity of the subject. As Palmer makes clear, it was an untidy system, and easy assumptions that the laws were of little remaining significance by the time of repeal, or, conversely, were a necessary adjunct to Corn Law abrogation in order to free British trade from anachronistic constraints, do not stand up to the detailed examination Palmer gives them in her first chapters on the relationship of protection to shipping in the first half of the nineteenth century (no attempt is made to look at the earlier workings of these laws). After 200 years, the laws still shaped the shipping industry in numerous if sometimes paradoxical ways, despite a series of exceptions created by several reciprocity treaties and also despite the fact that the "interest" of the industry, meaning its lobbying power in Parliament, was neither as large nor as unified as was, or is, commonly thought.

If the laws were complex, the question of repeal was equally so. Even free traders were willing to accept the assumption that without protection the mercantile fleet might not have been able to provide the "nursery of seamen" necessary to outfit the navy, the nation's first line of defense. Adam Smith himself called them perhaps "the wisest of all the commercial regulations of England" (p. 71). Cobdenites argued against the system, but because they were internationalists, not because they doubted the link between trade and legislative protection. Thus, repeal was not the result of coherent reformist ideology. Nor was it caused by technological revolution (faith in steam, for example), or socioeconomic change (the concentration of ownership in the hands of a few), or foreign pressure (the United States, in particular, was growing more protectionist). Sailors and shipwrights turned out in public demonstration, it is true, but in support of the laws, not their repeal.

By a process of elimination, Palmer demonstrates that while a few saw that the growth of trade did not

necessarily require protection, the main cause of repeal was short-term political interest, the need of the government of the day for a visible "liberal" success. Even then, reform was partial; regulations on the important coasting trade and the arming of ships remained untouched. Palmer's concluding chapter carries the story onward to 1860, making the point that it is difficult to show any clear-cut impact on the industry, either positive or negative, from repeal.

In her preface Palmer expresses the hope that her work "may go some way to support the growing claim of maritime history to be considered a field of historical study in its own right" (p. x), and indeed it does. But it is not simply her study of the shipping industry, nor the Navigation Laws, that validates this claim, but rather the judicious blend of often quantitative economic with sociopolitical history in explaining how and why repeal came about that makes this book a landmark study of interest to students of nineteenth-century British and maritime history alike.

BRITON C. BUSCH
Colgate University

LIONEL ROSE. *"Rogues and Vagabonds": Vagrant Underworld in Britain, 1815-1985*. New York: Routledge. 1988. Pp. ix, 254. \$65.00.

Lionel Rose is already well known for his stimulating and chilling account of infanticide in Britain during the nineteenth and twentieth centuries (*The Massacre of the Innocents* [1986]). This new book explores another crucial but elusive social problem, that of vagrancy, again over a wide period. From the demobilizations of the Napoleonic wars to the inner-city dislocations of the current day, Rose charts the changing pattern of vagrancy and the efforts of the state to deal with this seemingly intractable problem.

Rose's figures suggest that the nineteenth-century vagrant population contained a regular stratum of ex-servicemen and seasonal laborers, especially Irishmen, and that the proportion of women apprehended as vagrants declined over time, constituting no more than 20 percent by the middle of the century. He also argues that child vagrancy had declined by 1900, at least in England, a trend attributable more to the falling birth rate than to the vigilance of the National Society for the Prevention of Cruelty to Children and the school authorities. Young ex-servicemen continued to feature prominently in the tramping population of the twentieth century, but during the interwar years the relationship of vagrancy to unemployment, demobilization, and the huge reservoir of casual labor that was a necessary complement to industrial capitalism became less transparent. Although the drift from the distressed areas increased metropolitan vagrancy during the Great Depression, some of the homeless hailed from broken homes or were simply social

misfits. Only in recent years has the connection between unemployment and homelessness again become clearer.

Rose offers few firm explanations for this changing pattern. His approach is largely descriptive and impressionistic. While he throws some interesting light on some aspects of vagrancy—on the subculture of tramping, for example, or on the casual wards and the availability of cheap housing—broader generalizations are avoided. Part of the problem lies in the nature of the subject itself, in the fact that vagrancy was a convenient catch-all category for diverse forms of social marginality that could vary dramatically over time. Rose never confronts this issue head on. Had he done so, he would have been more attentive to the ways in which vagrancy was constructed over time, to the agencies that defined the discourse, and to the regulatory mechanisms that were designed to contain the problem. The study of vagrancy cannot be divorced from administrative practice and changing social imperatives. While Rose moves toward this conclusion, he does not develop its implications.

For instance, Rose's brief account of the mendicity societies never mentions their crucial role as the semi-official experts on vagrancy, forewarning the public against indiscriminate charity and casting itinerant poverty as socially and morally reprehensible even though their own reports revealed important correlations between vagrancy and unemployment. Such an intervention was critical to the undermining of the discretionary policies toward vagrancy that prevailed in the eighteenth century. Similarly, Rose's portrayal of the picaresque literature of vagabondage never addresses the question of why Victorians chose to see vagrancy this way, how it fit into their conception of the "dangerous classes" and how it reinforced their own moral rectitude about the dissolute urban "under class," its affront to industry, and its proclivity to disease. If Victorian "vagabondiana" threw oblique light on the subculture of the rookery, it also spoke to the structural underemployment of an undercapitalized economy, to the philanthropic anxieties of the middle class, and to the regulatory drives of the state.

The failure to draw out these connections results in a frustrating book, one strung together within a loosely thematic and chronological framework, with a bewildering array of subplots and mini-chapters. Rose's study is rich in detail, drawing widely from printed sources, but it is underanalyzed. Historians will find it a useful reference for many aspects of vagrancy and homelessness, right down to the contemporary controversies over the "sus" laws and the current inner-city lodging crisis. Yet they will not come away with a clear idea of how the problems of vagrancy informed and intersected with the prevailing patterns of British social policy over the past 150 years.

NICHOLAS ROGERS
York University

MARTIN J. WIENER. *Reconstructing the Criminal: Culture, Law, and Policy in England, 1830–1914*. New York: Cambridge University Press. 1990. Pp. ix, 381. \$38.50.

Studies of Victorian crime have multiplied rapidly in the last decade but many have been narrowly focused. Martin J. Wiener's book provides an intellectual framework for understanding the varieties and complexities of the topic by considering attitudes and actions in their cultural settings. Borrowing methods from literature and using a wide range of sources, Wiener gives coherence to the practices of nineteenth-century penology and a foundation to those of the twentieth century. In so doing, he offers a purview within which interpretations of pragmatism, humanitarianism, and social control can fall. Although he limits his inquiry to the shared values of national policy makers, he demonstrates how their ideas about the criminal developed and, more importantly, how their programs influenced new perceptions.

Separating early and late Victorians, Wiener reveals the fears of the former about crime as rising and violent, and as morally retrogressive in an era of material advance. Inspired by utilitarian and evangelical thinking, contemporaries attributed much law-breaking to individual recalcitrance but believed that accumulated acts of gratification would halt civilization. Crime therefore became "a central metaphor for disorder" (p. 11) and its discourse ethical. These generations, Wiener concludes, logically stressed character as a means to subdue potentially dangerous vitality. To encourage a habit of self-restraint, laws were to be clear and intent-driven; police, reminders of the merits of compliance; trials, impersonal routines culminating in consistent sentences; and prisons, frequently painful regulators of mind and body. This process, as Wiener correctly shows, took little account of human differences but rather extended responsibility, of the insane and juveniles among others.

By the 1880s, however, crime was synonymous less with energy than with debility, less about will than about circumstances. This transformation Wiener illustrates well by contrasting melodramas to sensation and detective novels. Persuaded by biological and environmental research, statistics, and medicine, and less afraid of revolution, Victorians and then Edwardians replaced censure with compassion. Citing parallel trends in civil law, Wiener explains how criminal rules gradually reflected a concept of harm instead of blame. Procedures and penalties were belatedly adjusted as more attention was paid, notwithstanding the sway of Edmund DuCane, to convicts who were atypical. If the desire to help tempered the system, then this goal likewise led, as Wiener recognizes, to larger state authority, more bureaucrats, and perhaps a greater reforming of the person than the moralizers ever envisioned.

Wiener's cogent analysis is based on the reading of substantial evidence from the age and current scholarship. Although without a bibliography, the book has abundant, often detailed footnotes and a comprehensive index. Unhappily, the typographical errors are always distracting, occasionally misleading.

If this work leaves the scrutiny of class differences to others, it nonetheless examines the criminal from a broad perspective. This approach, blurring conventional categories of investigation, is a welcome alternative to intense specialization. Wiener has thus not only "reconstructed" his subject but also suggested another dimension for future research. These results are significant for both history and historiography.

E. M. PALMEGIANO
Saint Peter's College

PATRICK BRANTLINGER. *Rule of Darkness: British Literature and Imperialism, 1830–1914*. Ithaca: Cornell University Press. 1988. Pp. xi, 309. \$29.95.

Patrick Brantlinger's study of the ideology of racism and imperialism in British literature between 1830 and 1914 is both impressive and disturbing. It is impressive in its comprehensive treatment of Victorian fiction set within colonial contexts as diverse as the West Indies, Canada, Australia, India, the Middle East, and Africa. Furthermore, this discussion is informed by literary studies of colonial discourse and the historiography of imperialism. From this breadth of scholarship, Brantlinger provides a cultural history of the ideology of imperialism in which the predominant theme is a disturbing and continuous record of unmitigated racism.

The book is organized into three sections: "Dawn," dealing with the origins of this literature in the 1830s and 1840s; "Noon," the book's largest section, treating the period of ascendancy from the 1840s to roughly 1900; and "Dusk," concerning the period of decline after 1880. Within these sections are a number of interesting individual studies. Tourism made travel seem less heroic and more routine. In the case of one tourist to the Middle East, Benjamin Disraeli, his travels greatly influenced his views on race and empire. Another chapter, on "Imperial Gothic," links imperial adventure fiction with the birth of the spy novel and the science-fiction thriller.

Brantlinger rejects a narrow political definition of imperialism, with its distinctions between formal and informal empire, seeing imperialism as an ideology of domination that may take a variety of political, economic, or cultural forms, but which incorporates a pride in the British empire and a belief in the Victorian civilizing mission. Unfortunately, a precise definition of racism is not provided, but it would appear to include any stereotyping of individuals or groups by racial category and any presumption of racial superiority.

Within this broad framework, Brantlinger sees a

shift from early Victorian optimism about the conversion of the world to British and Christian ways to a growing pessimism, from the 1850s onward, which doubted whether savages could be converted and worried about British imperial decline. As this pessimism intensified, so did Victorian racism. Imperialism came to rely on the territorial conquest of formal empire and on a Social Darwinian rationale to justify the subordination of non-Europeans. The prevalence of racism is accounted for by the psychology of prejudice. The Victorians projected their repressed guilt, fear, and anxiety on nonwhite peoples of the empire who were envisaged as the oversexed, sadistic, dishonest, idle brutes of the Victorians' imagination.

This picture of Victorian racism is disturbing. The Victorians are charged with blaming the victim for the outrages of imperialism, but cannot the psychology of projection be turned on ourselves as well? Instead of blaming the victims we blame the Victorians, creating a stereotype of the Victorian racist on whom we can project the guilt for our own racism. What is the evidence for the psychology of projection at work in the minds of these Victorian authors? Can we really slide so readily from the views of novelists to generalizations about the attitudes of readers? Nor are we dealing solely with the world of imagination. As Brantlinger fully realizes, but underplays through his psychology of projection, the atrocities of the Indian Mutiny, the bloody carnage inflicted on the Maoris, the judicial murders that suppressed the Jamaican insurrection, and the decimation of heroic Zulu warriors before the Gatling guns, were historical, not imaginary, events. The Victorians responded to these events not with one voice but with divided counsels.

The Victorians not only gave birth to our racism but also to our antiracism. For example, we are told that John Stuart Mill shared his father's view of the incapacity of Indians for self-government, and thus that the principles espoused in "On Liberty" or "Representative Government" do not apply to nonwhites (p. 23). Where is the John Stuart Mill who provided the antiracist rebuttal to Thomas Carlyle's "Occasional Discourse on the Nigger Question"? At the very time that Mill pressed for extending the vote to the working class and to women, he wrote in favor of enfranchising the freed slaves in the United States. Mill and Harriet Taylor were so confident of the abolitionist principles of their readers that they used the analogy of race and gender to argue against biological determinist defenses of the subjection of women. If we look closely at the discourse of antiracism, we find the following terms all come into use in the period between 1830 and 1914: color prejudice, anti-Semitism, discrimination, segregation, race relations, and racialism.

As the nineteenth century advanced this antiracist voice grew weaker but it was never extinguished. Brantlinger's cultural history overlooks this voice, for understandable reasons. It is a voice almost absent

from the literary tradition. Following on Raymond Williams's question about class, we need to explain the limited sympathies of the literary tradition on the question of race. The blame should not be cast on the reading public. Much to the chagrin of Charles Dickens and Anthony Trollope, the publishing and theatrical success story of the nineteenth century was American and not English. *Uncle Tom's Cabin* dealt with race in a complex and ambiguous fashion, but nonetheless excited Victorian sympathies with a message of liberation from racial oppression.

One cannot help but be impressed by Brantlinger's comprehensive treatment of the literature of empire. My concern is that this book confirms a one-dimensional myth about the uniformity and universality of Victorian racism. If the purpose of the study is to help us better understand the racism of our own time, we would be better served by a more complex cultural history that explores the diversity of Victorian opinions between the poles of the predominant racist literary tradition and the minority antiracist voice.

DOUGLAS A. LORIMER
Wilfrid Laurier University

MICHAEL WHEELER. *Death and the Future Life in Victorian Literature and Theology*. New York: Cambridge University Press. 1990. Pp. xv, 456. \$54.50.

The four last things—death, judgment, heaven, and hell—were subjects of obsessive concern and controversy in Victorian England. The history of the theological debate has already been well studied by Geoffrey Rowell in *Hell and the Victorians* (1974). Michael Wheeler builds on Rowell's work in examining the literary implications of the subject. His book is primarily a literary study, but in the detail of its extensive research there is much that will interest historians.

In the first part of the book, Wheeler examines Victorian ideas on each of the last things through a wide range of sources and media: deathbed scenes (proper Victorians died up to expectations), graveyard scenes, biographies, hymns, tales and novels, poems (especially epics such as Edward Bickersteth's *Yesterday, Today, and For Ever*), and art (the book contains eighteen illustrations). This wide sampling, especially of unfamiliar works, is his most useful contribution to history. Wheeler finds that the unclarity of the New Testament and the Book of Common Prayer on the future life encouraged a variety of orthodox and unorthodox interpretations, especially with regard to the possibility of an intermediate state. He finds the central problem to be one of the inadequacy of language: "it is at the very point of stress or fracture in the discourse of consolation that both the provisional nature of language and communication and the grounds of Christian faith are laid bare" (p. 21). There was an increasing tendency to spiritualize

and internalize both heaven and hell and to abandon the belief in the *poena sensus* of damnation.

The second part of the book is a revisionist reading of four central literary texts: Alfred Tennyson's *In Memoriam*, Charles Dickens's *Our Mutual Friend*, John Henry Newman's *The Dream of Gerontius* (Edward Elgar's musical setting is also studied), and Gerard Manley Hopkins's *The Wreck of the Deutschland*. The first two represent Broad Church liberalism; Tennyson, the poet of the "larger hope" of universal salvation, is revealed as more coherent and Dickens as more religiously knowledgeable than is generally recognized. The second pair are Roman Catholics, writing as Catholics to Catholics, imbued with the orthodoxy of their Church and informed by its liturgy and, in Hopkins's case, by works such as the *Spiritual Exercises*. These are great but not representative writers, and this part of the work is therefore of literary rather than historical interest. A historian may be allowed to correct an error (p. 312): *The Dream of Gerontius* was accepted for publication not by Newman's Jesuit friend Henry Coleridge but by the first editor of *The Month*, Fanny Margaret Taylor.

JOSEF L. ALTHOLZ
University of Minnesota,
Minneapolis

THOMAS RICHARDS. *The Commodity Culture of Victorian England: Advertising and Spectacle, 1851–1914*. Stanford: Stanford University Press. 1990. Pp. xiv, 306. \$29.50.

This book concerns the history of commercial advertising in Britain between the Great Exhibition of 1851 and World War I. Theoretically, Thomas Richards has done a lot of fashionable reading (especially the works of Karl Marx and Jean Baudrillard) and in the semiotics of advertising and "spectacle." Empirically, he has done his homework, reading the relevant historical monographs. The end result is a book that does not, theoretically, take us much farther than E. S. Turner's *Shocking History of Advertising!* (1953), a book written for a popular audience.

Richards illustrates five examples of the extension of advertising art and artifice during the years before World War I—the Great Exhibition of 1851 (housed completely in Sir Joseph Paxton's commodious Crystal Palace in Hyde Park), Queen Victoria's Jubilee of 1887, "selling Darkest Africa" in the exploits of Henry Stanley and the Boer War, merchandising patent medicines in Victorian Era, and the later role of female advertising needs (and prurient charms) in a chapter aptly called "Those Lovely Seaside Girls." Whether or not these advertising gimmicks, "outbursts of bourgeois self-congratulation," were "sucking" British consumers "into the vortex of a master-slave dialectic" (p. 7), or have as much importance to Victorian culture as a whole as the author thinks, is another thing.

For my part, I lean more toward the statistical evidence of economic history. For example, Lorna Weatherill's *Consumer Behaviour and Material Culture in Britain, 1600–1760* (1988), where she makes a useful distinction, in an earlier period of British history, between social hierarchy and consumption hierarchy, seems to me to make much more historical sense than Richards's "the experience of consumption," which ultimately became an "all-encompassing" realization "inseparable from the knowledge of the self" (p. 7).

PETER D'A. JONES
University of Illinois,
Chicago

ROSS MCKIBBIN. *The Ideologies of Class: Social Relations in Britain, 1880–1950*. New York: Clarendon Press of Oxford University Press. 1990. Pp. ix, 308.

The essays in this book (all except one previously published) amount to a powerful empirical and theoretical examination of the relationship between the working class and society. Ross McKibbin explores the interplay between the interior structures of working-class culture and the external forces that confronted it during the period of its classic sociological presence. Unlike other, more self-consciously theorizing, historians, McKibbin is not obsessed with the failures and absences of class action. McKibbin's working class is neither beleaguered nor regressively defensive, but one that maximizes its potential for cultural and political development within the constraints of contemporary social relations and historical heritage.

Three of the essays focus on the nature of the associational culture of the working class. In "Why Was There No Marxism in Great Britain?", and in the essays on working-class gambling and sports and hobbies, he argues that working-class culture was neither separatist nor rejectionist but provided an alternative sphere of activity to politics. Within this sphere a richly textured intellectuality was constructed, as evidenced by the purposeful acquisition of knowledge necessary for working-class men (it should be noted that women are largely absent from this account) to intelligently read and play betting forms or develop hobbies. Such activity constituted an autonomous system of knowledge with its own codes, rivalries, and achievements that were not available to outsiders. The autonomy of working-class culture was largely revealed in such spheres.

But autonomy was also dependent on forces external to the working class. The space secured was a function of the dominant practice of the state that assumed a basic "neutrality" toward labor. This was particularly true in capital-labor relations and in political economy where the dominant *laissez-faire* posture separated politics and economics in a way not found in other countries.

Indeed, in a sense the spheres of independence that were created within working-class society were a condition of its integration into civil society. If the state's neutrality provided freedom for labor, then it also allowed labor to accept the legitimacy of governing institutions. A rejectionist political ideology like Marxism, therefore, had less relevance in Britain because the state could not viably be portrayed as an interventionist force. Even when faced with the powerful arguments of certain social reformers who wished to root out the dysfunctional elements in working-class family life (as discussed in his essay "Class and Poverty in Edwardian England"), the actions of the state were generally restrained.

The essays on labor politics again focus on the reciprocal relationship between working-class politics and state structures. In "The Franchise Factor and the Rise of the Labour Party," McKibbin (and his collaborators) decisively demonstrate how the growth of the Labour Party after World War I owed much to the way the act of 1918 removed the legal restraints on the size of the electorate. In an important revisionist essay on the economic policy of the second Labour government, McKibbin demonstrates that the government went about as far as it could in a reflationary direction, given the absence of political or theoretical alternatives and facing opposition to a reconstruction policy that had been forged within the political establishment in the early 1920s.

An essay on the roots of Conservative political hegemony in the interwar years shows how this opposition rested precisely on the recognition that such a policy implied a reordered political economy that would have greatly strengthened Labour's presence. Conservative politics therefore consciously mobilized support around an anti-working-class imagery that was dissolved only when the crucible of 1940–41 created the necessity to integrate the working class into the political economy.

A short review cannot do justice to this book; taken together these essays reveal a refreshingly sensible conception of working-class history as making itself and helping to make the conditions within which it exists.

RICHARD PRICE
University of Maryland,
College Park

MICHAEL C. C. ADAMS. *The Great Adventure: Male Desire and the Coming of World War I*. Bloomington: Indiana University Press. 1990. Pp. xv, 168. \$22.50.

There remains something deeply mysterious about the Great War. Why was its outbreak met with such enthusiasm? Why did the soldiers go on fighting after they realized that there was little, if any, chance of victory and the stakes of defeat were so high? Why was this immensely destructive war followed twenty years later by one even more devastating? Common

sense suggests that Europeans would have done anything necessary to avoid the repetition of such a tragic event.

Michael C. C. Adams thinks that he has the answer to these questions. The secret lies in gender. The clash of soldiers was preceded by the separation of the sexes. During the nineteenth century the roles of men and women became more differentiated; fathers spent less time at home; and boys were brought up to believe that "manliness" expressed itself in sport, hunting, killing, and war. War, they were taught, would cleanse a society corrupted by materialism, restore chivalry and knightly values, and relieve the unbearable boredom of an increasingly commercial and bureaucratic society in which the sense of individualism was being lost. In short, war was "the great adventure" that would make it possible for young males to escape from capitalism and the company of women and play the most thrilling game of all in which the stakes were life or death and killing could be dignified as the performance of one's patriotic duty. Taking issue with what he conceives to be the orthodox view, Adams goes on to maintain that even after having experienced the war's horrors and coming to view themselves as victims, "Lost Generation" writers went on to romanticize it in their books, thus keeping alive its allure and helping to prepare the sacrifice of the younger generation.

This argument is unfurled with great verve, intensely felt and sometimes movingly passionate, and a wealth of examples from well and lesser-known sources. The result is an entertaining and cleverly constructed book that pays respectful homage to American feminist accounts of nineteenth and twentieth-century history. But is it convincing? No one would dispute that many soldiers of World War I welcomed the call to arms and later looked back on their years at the front with nostalgia. Nor is the misogyny of much early twentieth-century European culture in doubt. But can all this be explained, as Adams claims, by the separation of sex roles, the repressive practices of middle and upper-class educational institutions, the cult of games, and a flight from women, represented in the unconscious by the omnipresent and suffocating mother who is both desired and abhorred? Perhaps in the case of some of those English public schoolboys who dominate Adams's pages. But what about those equally dedicated and nostalgic soldiers in other European countries who fail to fit Adams's public school model? Would war have been avoided had late-nineteenth-century patriarchy spent more time with their sons and bid them to read poetry instead of playing rugby? Is it not possible that some World War I soldiers fought because they thought that they were standing up for right against wrong, defending their country against its mortal adversary, or perhaps just out of a "lonely impulse of delight?" These are just a few of the many

questions one takes away from Adams's engaging and suggestive book.

ROBERT WOHL
University of California,
Los Angeles

DAVID E. OMISSI. *Air Power and Colonial Control: The Royal Air Force 1919–1939*. (Studies in Imperialism.) New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. xvi, 260. \$79.95.

In this book, the latest in a series under the heading "Studies in Imperialism," David E. Omissi describes two separate but interrelated processes: the use of aircraft to maintain British power on the fringes of the empire between the world wars and the survival of the Royal Air Force (RAF) as a separate service. In both cases, his claims are restrained and backed with well-documented evidence.

In 1919, Britain encountered resistance not only in its old imperial territories but in its new mandates as well. Rebellions broke out in Mesopotamia among the Arabs of the south and among the Kurds of the north. The 1920s also witnessed uprisings in Somaliland, the Aden protectorate, the Sudan, Transjordan, and the northwestern frontier of India. Caught between these challenges and a severe budget crisis, the cabinet accepted Winston Churchill's idea that the RAF could uphold British hegemony in the empire more cheaply than any other means.

This policy succeeded in Kurdistan, Aden, Somaliland, and Transjordan, all desert, swamp, or mountain areas whose inhabitants "could, by reason of their remoteness, martial qualities and poverty, escape both the benefits and the drawbacks of civilization" (p. 211). Air control failed in settled areas like Ireland and Palestine, where aircraft were too blunt an instrument and resistance took the form of mass movements rather than guerrilla warfare.

Unlike more ardent enthusiasts, Omissi does not claim that air power prolonged the British empire. He is careful to analyze its strengths and weaknesses, in particular the geographical limitations under which aircraft could be effective and the remarkable adaptations of tribal peoples to air attack.

When he turns to the role of imperial air control in the domestic politics of interwar Britain, he makes a stronger claim. Although colonial air warfare contributed little to air strategy or technology, it did ensure the survival of the RAF: "Had the doctrine of 'control without occupation' not been successfully developed and applied in Iraq . . . the Royal Air Force would not have survived long into the 1920s . . . even had an independent service been recreated in the 1930s, it is doubtful if it would have attained the same degree of excellence demonstrated by Fighter Command in the defensive battles against the Luftwaffe in 1940" (p. 211).

In his final chapter Omissi draws our attention to

other, similar cases between the wars, especially the French in Syria and Morocco and the Italians in Libya and Ethiopia. Air control seems to have caused more ethical dilemmas in Britain than elsewhere. Whereas the British "could not afford the political costs of excessive violence," Italian fascists "seemed at times to make a virtue of a brutality that was more systematic than the occasional excesses of the democracies" (p. 209).

Colonial air control is an aspect of a larger story, one found in embryonic form in Omissi's book but, alas, never developed. He does mention "successor states" among the users of aircraft for domination, and, in the conclusion, states that "The emergence of the aeroplane as a weapon to enforce government demands irreversibly altered the balance of power between the central state and the societies on its geographical margins" (p. 211). Since the relations between central states (or "civilizations") and their geographical margins (or "barbarians") has been a theme of world history for over six millennia, and since technological change has played a large part in these relations, Omissi's book should be appreciated not only for its contribution to British empire historiography but also in the context of long-run and global history.

DANIEL R. HEADRICK
Roosevelt University

THOMAS R. MOCKAITIS. *British Counterinsurgency, 1919-60*. New York: St. Martin's. 1990. Pp. x, 210. \$45.00.

In this work, Thomas R. Mockaitis develops the thesis that the United Kingdom was the most successful nation confronting insurgencies in the post-World War II era. He asserts that between 1919 and 1960 Britain developed the principles of and techniques necessary to defeat an insurgency. He defines insurgencies as political and ideological movements whose goals are the seizure of power so as to produce political, social, and/or economic change, if not revolution. This distinguishes them from earlier indigenous guerrilla movements of various states, whose main purpose was the expulsion of outside military forces in temporary occupation of their territory, or operations to suppress banditry or lawlessness. To achieve success, the techniques Britain used rested on three principles: use of minimum force, based on the principles of English Common Law; flexibility in operations, that is, decentralization and small unit operations; and close military-civil cooperation, including military, police, and civil government agencies at all administrative and operational levels. The latter principle had the benefit of ensuring proper intelligence necessary to defeat an insurgent, placing an insurgency outside the realm of conventional military operations and within that of security ones, and developing a proper political program to identify

the grievances of a people and provide a remedy for them; this constituted, Mockaitis repeatedly says, winning "the hearts and minds of the people," a phrase he notes first appeared in print in 1932 but which itself was a quote from a nineteenth-century author. But the theory of counterinsurgency is deceptive: issues associated with insurgency in general and developing a plan to defeat one are always easy in the abstract, as are reflective analyses of past insurgencies. Mockaitis acknowledges the former and would agree with the latter statement.

To Mockaitis, Britain's colonial administrative structure and the British army and its regimental organization, traditions, and colonial constabulary mission helped ensure that "lessons learned" between 1919 and 1960 were gradually passed from one locality to another and eventually refined. This occurred, however, in an ad hoc, personalized, and informal manner, without the development of a formal doctrine and with limited inclusion of such issues into the professional military education system of the time. In developing these themes, Mockaitis traces their evolution through nine disturbances or emergencies: the Anglo-Irish War, the Moplah Rebellion, the Arab Revolt, actions on the Northwest Frontier, Burma, the Palestine crisis, Malaya, Kenya, and Cyprus. Ironically, to "win" the British had to ultimately determine, accept, and then grant the main underlying issue afflicting the many diverse areas where they confronted insurgencies after World War II: independence. In so doing, although British rule would end, London could determine who came to power. This also reveals why the continual conflict in Northern Ireland remains unresolved to this date.

This book is the result of a massive array of research in primary official records, interviews, and correspondence, plus a thorough foundation in secondary literature and memoirs. It is introduced by a superb chapter with conceptual definitions of words so often used interchangeably (guerrilla, terrorism, insurgency), followed by the development of the conceptual view. I would have preferred his organization to be by state or locality, spread throughout the time period covered, as opposed to the thematic one Mockaitis used. His organization causes a repetition of facts and often phrases. This is, however, a minor criticism of a succinct but superb work.

Woven into his account is the implied comparison with French and American efforts in counterinsurgency. The French were defeated in their colonies through the application of too much force, while the United States was beaten through a reliance on technology and firepower. One last comment should be noted about Mockaitis's analysis of the British Army. He correctly notes its long role within the British empire of the nineteenth and twentieth centuries, and the positive and negative aspects of its regimental system. In his view, this permitted the army to develop an approach to "irregular warfare" that its counterparts in Europe and the United States did

not. Stated another way, the role of the British army in World War I and World War II were aberrations from its normal role and heritage. His insightful comparison between the British and American armies (pp. 173–76), whereby alleged virtues of the U.S. army are in reality a weakness while the frequent criticisms of the British army are a strength, is must reading. This is an ideal volume for use in seminars on the topic of post–World War II insurgencies, or modern military history courses. It should be noted that Mockaitis has published a journal article summarizing, and expanding on, his ideas. See “A New Era of Counterinsurgency,” *The RUSI Journal* 136 [1991], 73–78.

DONALD F. BITTNER

*Marine Corps Command and Staff College
Quantico, Virginia*

P. M. H. BELL. *John Bull and the Bear: British Public Opinion, Foreign Policy and the Soviet Union 1941–1945*. New York: Edward Arnold; distributed by Routledge, Chapman and Hall. 1990. Pp. x, 214. \$45.00.

In this volume, P. M. H. Bell explores wartime British public opinion regarding the Soviet Union. His focus is on government efforts to influence opinion through propaganda and censorship in the interest of foreign policy. Bell's work is first-rate, based on a thorough reading of government documents and the writings of other historians on wartime propaganda.

Bell argues that while the government knew the importance of public support in prosecuting a prolonged war, there was no way to ensure that support. His example is Whitehall's attempt at persuading the British to reject two decades of anti-Bolshevik propaganda and accept the Soviet Union as an ally. After setting out the parameters of a propaganda-censorship-foreign policy framework, Bell details three “case studies” in which the Soviet Union presented a negative image to British opinion: the Katyn graves revelation in 1943, the Warsaw uprising in 1944, and the Yalta debate in 1945. Each instance contained what appeared to be brutal, unscrupulous, or calculating Soviet behavior, or all three, which offended British opinion. The British government's challenge was to counteract the effect of these events on public opinion through propaganda and censorship.

The study ends with Bell's conclusion that “there is little sign that opinion in the country could be moulded at the government's will, though much evidence that it could be encouraged to move in a direction which it already wished to follow” (p. 187). While this is no new discovery in itself, it has not heretofore been applied to Britain's propaganda policy in aid of the Anglo-Soviet wartime alliance.

Bell's research draws on Cabinet, Foreign Office, Ministry of Information, and public polling records to good effect. In particular, he examines how propaganda was assessed, which is to say, how its effects

were measured. He examines Gallup polls, Mass-Observation, Ministry of Information home intelligence, and BBC intelligence surveys, and concludes that there was, then or now, no way to be absolutely certain that public opinion was shaped by official propaganda. Most historians of propaganda have drawn the same conclusion, although on less-substantive evidence. Bell demonstrates that their instinct was correct.

Otherwise, Bell develops no new approach to the study of his subject, nor is he the first to see the importance of connections between propaganda and foreign policy. What he has done is write an excellent monograph that examines government propaganda and censorship policies as they worked to promote the Soviet alliance. In doing this he uses criteria for studying propaganda and foreign policy developed by Philip M. Taylor, among others. Bell's tendency to use passive voice, which occasions the undergraduate device of adding “that of” as lead-in to the subject of the sentence is only a minor annoyance. On the whole, this work is an important addition to the history of British propaganda in wartime.

ROBERT COLE

Utah State University

CLIONA MURPHY. *The Women's Suffrage Movement and Irish Society in the Early Twentieth Century*. Philadelphia: Temple University Press. 1989. Pp. ix, 233. \$34.95.

Cliona Murphy's book is an important addition to the small yet expanding list of works illuminating the hidden history of modern Irish women. Drawing on a wide range of archival, manuscript, newspaper, and journal sources, the author provides a vivid account of a campaign that heretofore has been overshadowed in the historiography by the English suffragist movement and the controversies surrounding the third Home Rule Bill.

Murphy documents the varied sources from which Irish suffragism drew its inspiration, ranging from Mary Wollstonecraft's and Florence Nightingale's Irish links to the militant example of Anna and Fanny Parnell and the Ladies Land League during the Land War of 1879–81. The activities of English feminists such as Annie Besant and the Pankhursts further served to raise the feminist consciousness of Irish women. The democratization of Irish local government between 1870 and 1898 and improved opportunities in higher education were also important in stimulating Irish women to seek the parliamentary franchise during the first decade of this century. Although the movement was solidly based in the middle class, and especially among educated Dublin intellectuals, there is evidence of support from rural women and the ranks of labor, so that there were an estimated 3,000 activists by 1912.

A good account is given of the religious and political obstacles that Irish suffragists faced in building a

broadly based campaign. While most Catholic clergy opposed female participation in parliamentary politics as potentially corrupting family values, Murphy cites some clerical support for female suffrage as a means of combating the evils of poverty, drink, and prostitution. Political barriers proved the most difficult for the suffragist movement. With Home Rule hanging in the balance in 1910–12, the suffragist loyalties of Irish women were severely strained. Many reordered their priorities to put the cause of nationalism or unionism first.

Not prepared to let the constitutional issue take precedence over their goal, more-militant suffragists established the Irish Women's Franchise League (IWFL). This group, whose prominent leaders included Hanna Sheehy-Skeffington, Frank Sheehy-Skeffington, Margaret Cousins, and James Cousins, guarded its independence from both the Irish Parliamentary and Unionist parties, as well as from the imperialist tendencies of the Pankhursts' Women's Social and Political Union.

Murphy provides a detailed account of IWFL activities in the period 1911–14, including press campaigns and large public demonstrations as well as physical assaults on the persons and property of prominent nationalist leaders such as John Redmond and John Dillon. As in England, these militant tactics resulted in arrests, convictions, and hunger strikes similar to those mounted by the English suffragists. Murphy suggests that the claim for political status asserted by the incarcerated Irish suffragists provided a precedent that male republican prisoners would emulate in the aftermath of the 1916 rising.

The author's analysis of the strained relations between the militant suffragists and the Irish Parliamentary Party is critical but essentially balanced. She explains the IWFL's conviction that Westminster was more likely to grant the franchise than a Dublin parliament, and why their organization was reluctant to have their cause held hostage to the uncertain fate of Home Rule. Noting the very traditional attitudes of the Irish party leaders, the author explains Redmond's legitimate fears that the suffragist campaign might precipitate a Liberal collapse and the consequent loss of Home Rule, or at the very least a reduction of approximately forty Irish seats if any franchise bill was introduced prior to the passage of Home Rule.

The outbreak of war in 1914 was another blow to the Irish movement, although members of the IWFL vigorously opposed the war as female pacifists. The aftermath of the Easter Rising was to dramatically change the political landscape in Ireland, and the suffragist cause was subsumed within republicanism. By 1918 the war and coalition politics had eroded the opposition to extending the parliamentary vote to women, and Irish women were enfranchised along with their English sisters in 1918.

For much of the twentieth century, the political and cultural climate in Ireland has not been hospitable to

the kind of independent feminism espoused by the IWFL and its supporters. The election of the Labour Party candidate Mary Robinson as president of Ireland in November 1990, however, suggests that Irish women, and some Irish men, have not forgotten the example of an earlier generation of Irish feminists. Murphy's book will be welcomed by students of Irish history as well as by those involved in women's studies.

CATHERINE B. SHANNON
Westfield State College

HELGA WOGGON. *Integrativer Sozialismus und nationale Befreiung: Politik und Wirkungsgeschichte James Connollys in Irland*. (Publications of the German Historical Institute, London, number 24.) Göttingen: Vandenhoeck und Ruprecht. 1990. Pp. 517. DM 142.

The seventy-fifth anniversary of the execution of James Connolly on May 12, 1991, passed virtually unnoted in the United States, where he helped to create the socialist movement, and it received scant attention in Ireland, where he gave his life to found the Republic. This neglect was less the result of ignorance or indifference than of change in fashion, since Marxism seems irrelevant to many Americans, and many of the Irish have grown weary of a nationalism that embraces revolutionary violence.

Born in 1868, Connolly spent his formative years among the Irish laboring population in Britain, became an agitator, union organizer, and journalist in Ireland, and, between 1903 and 1910, lived in America, where he helped to organize the Socialist Labor Party and the Industrial Workers of the World. Returning to Ireland, he combined leadership of the trade union movement with support for separatist nationalism. As one of the principal leaders of the Easter Rising of 1916, he paid the supreme penalty for its failure.

Helga Woggon has written the first German-language biography of Connolly, making an important contribution to a better understanding of a man not too well served even by writers in his own country. Based on massive archival research, personal interviews, and a comprehensive analysis of secondary works, her study places Connolly in the context of Ireland's history and the socialist vision of his day. Although this slum-born worker was largely self-educated, he became a prolific author of pamphlets and essays, and his major work, *Labour in Irish History* (1910), reveals his special sense of his nation's growth and destiny.

Woggon concentrates on Connolly's attempt to integrate socialism with the cause of Irish national liberation. Although a devoted socialist, he held that each country's distinctive experience and culture must determine the pattern of its social and economic emancipation. Ireland, in Connolly's view, must be a sovereign state, but a socialist one. A national revolu-

tion that merely exchanged the rule of British capitalists for that of Irish capitalists would achieve nothing.

While the "blood sacrifice" of Connolly and his comrades led ultimately to Irish independence, his worst fears were realized by the emergence of a bourgeois regime that has remained in place for the last seventy years. Those in Ireland who regret the failure of Connolly's vision will welcome an English translation of this book. Indeed, its ultimate availability to a wider audience, both in Europe and the United States, might have a particular relevance as we seek to resolve the conflicting claims of socialism and capitalism, of nationalism and internationalism, that have arisen to plague the closing decades of the twentieth century.

WILLIAM D. GRIFFIN
St. John's University
Jamaica, New York

CATHERINE RANDALL COATS. *Subverting the System: D'Aubigné and Calvinism*. (Sixteenth Century Essays and Studies, number 14.) Kirksville, Mo.: Sixteenth Century Journal. 1990. Pp. vii, 214. \$35.00.

Catherine Randall Coats's book examines the relationship between Calvinist theology and French literature in the late Renaissance through an examination of the works of Agrippa D'Aubigné. Calvinist teaching, as set out by Calvin's successor Theodore de Bèze, regarded literary self-expression not subordinated to Scripture with deep suspicion. Bèze and his followers conceived the task of the writer to be the explication of a truth that had already been revealed. Attempts by authors to invent a fictionalized world based on the imagination appeared to be a falling away from truth and a form of rebellion against divine authority rooted in self-glorification. It was within these rather narrow religious constraints that D'Aubigné, a committed Calvinist, began his career as a writer.

It is Coats's contention that D'Aubigné's development as an author can be understood as a creative progression from the stage of paraphrase through exegesis, exposition, and epic, and ultimately toward an unconstrained fictional model. D'Aubigné was able to accomplish this journey toward increasing self-expression while remaining faithful to Calvinism by a creative exploitation of the techniques of literary self-expression. In the *Méditations* (1588), a work of the early period of his career, D'Aubigné opened a creative space by engaging in a series of dialogues between the self and God in which the self was able to move through and beyond sin. D'Aubigné makes it appear that God allows writing because He permits and redeems the self through the succession of dialogues. In his *La confession du Sieur de Sancy* D'Aubigné (1597–1617), he self-consciously speaks through a

multiplicity of voices openly displaying the process of literary invention and at the same time compelling the reader to reenact this process while discovering the author behind it.

Literary craftiness thus allowed D'Aubigné to expand gradually the boundaries of what was theologically permissible. By the time of his last work, *Sa vie a ses enfants* (1629), D'Aubigné let loose an unrestrained subjectivity, not only liberated from theological constraints but also more truthful than the impersonal narration of events found in his *Histoire universelle* (1618). Coats concludes that this literary progress makes D'Aubigné one of the pioneers in the development of a modern sense of the self.

In carrying through this analysis Coats relies on a mix of the tools provided by theological and literary criticism, the latter based on an unobtrusive deployment of the insights of deconstruction. Taken on its own terms, Coats's work is a convincing demonstration of the ways that the creative writer can overturn the constraints even of the narrowest of established orders. Yet the historian cannot but wish that Coats had dealt more fully with the way D'Aubigné's subjectivity was colored by his sense of himself as a sacred historian and martyrologist. Likewise, one has difficulty recognizing a modern self in D'Aubigné, who was not only an author but also a nobleman, courtier, and warrior. A full understanding of his subjectivity is only possible when the relationships entailed by these social roles are fully explored.

HENRY HELLER
University of Manitoba

HARRIET G. ROSENBERG. *A Negotiated World: Three Centuries of Change in a French Alpine Community*. Foreword by ERIC R. WOLF. Buffalo: University of Toronto Press. 1988. Pp. xv, 234. \$30.00.

In the late 1960s Harriet G. Rosenberg was engaged in graduate work in sociology with Eric Wolf and Charles Tilly at the University of Michigan, so it was only natural that for her dissertation she should become interested in small community studies in Europe. In 1972 she received a grant from the Ford Foundation to carry out two years of field work in the area she had chosen to study, the isolated small communities of the highest Alps of southern France. There she had the good fortune to meet Robert Burns, Jr., who had made a study of the village of Saint-Véran a few years earlier. He introduced her to the region and advised her on the choice of a village in which to live. She chose the village of Abriès, a community of almost two hundred near the Italian frontier in the Hautes-Alpes department.

The author's interest involved especially the relationship of the village to the other communities of the region and to the nation over the years for which records exist. She wanted to understand how the organization, the policies, and the decisions of the

larger units affected the lives of people in this remote village. She was fortunate to find a vast documentation in various archives in the commune and collections in Gap, Briançon, and Paris. At the same time, she was living in the village and interviewing people, which was not too hard a task since there were fewer than two hundred people living there and they all seemed eager to cooperate.

This book, the report of her findings, is thus both historical and anthropological. Her emphasis is on social organization and economy, although she attempts to include some information on the daily lives of the inhabitants. Although the subtitle limits the scope of the study to the last three centuries, she also provides what information she has on developments since the thirteenth century. The book is divided into three parts—the *ancien régime*, the Revolution and nineteenth century, and the twentieth century with emphasis on the early 1970s.

The first important date in the history of the region is 1343 because in that year Humbert II, who governed the Viennois (the southeastern area of France with its capital in Vienne), sold a charter to the five Alpine valleys around the city of Briançon: "The agreement was that the communities would [make] an annual tribute payment, in exchange for exemption from future taxes and various rights" (p. 39). Five years later, the Dauphin Humbert sold his whole territory to the King of France and entered a religious order. The agreement of 1343, however, was accepted by France, so for the duration of the *ancien régime* the five valleys continued to enjoy these special privileges. These valleys were called *escartons*, and the whole area became known until the French Revolution as the *République des Escartons*. An elective system was set up, and all relations with the rest of France were negotiated by lawyers through the courts. In fact, then, the region was self-governing but under the control of the lawyers representing it. The system worked, and the *République des Escartons* prospered. During this period the population remained about 2,000.

The Revolution put an end to this system of *escartons*. The area became like all the rest of France, governed by a strongly centralized government. The inhabitants voted for their representatives, but they had lost their privileged situation. When Paris decreed that sheep destroyed forests, the Abriësois could no longer raise sheep. Paris was misinformed, but that did not change the decree. At the same time, as in most of France, a rural exodus was taking place. By the 1970s the population of Abriès had dropped from 2,000 to less than 200.

Unfortunately, this book was published fourteen years after the fieldwork, so we are left wondering what has become of Abriès. By the end of the book we become so involved with Abriès that we can only hope that this fragmented community has been revitalized

by summer tourism and winter skiing. It has, after all, survived many previous catastrophes.

LAURENCE WYLIE
Harvard University

BIANCAMARIA FONTANA. *Benjamin Constant and the Post-Revolutionary Mind*. New Haven: Yale University Press. 1991. Pp. xvii, 165. \$25.00.

In textbook accounts liberalism appears as a rejection of revolution and an embrace of Enlightenment ideals, shorn of any trace of democracy. Specifically, liberals favored free trade, governments with limited powers, and political participation by the propertied. In this guise liberalism appears as little more than the defense of the rich against labor's demands. Such was the judgment of the Left in the nineteenth century, increasingly unopposed as the Right found refuge in religion and nationalism. Liberalism, a burned-out candle, then received its modern incarnation in history books.

Recently, however, scholars (and some politicians) have been rehabilitating liberalism. Students of the French Revolution in particular have seemed to argue that the failure to adopt the tenets associated with liberalism proved the downfall of that eventful episode. Another part of this revival has been serious scrutiny of the liberal Benjamin Constant, who has been the subject of such new studies as the important volume by Stephen Holmes (*Benjamin Constant and the Making of Modern Liberalism* [1984]). Indeed, both of these books provide a view of Constant's liberalism that appears more nuanced than the stereotype.

Before directly considering how Biancamaria Fontana updates this older view of liberalism, it is worth examining her analysis of Constant. Fontana begins by asserting that the foundation of Constant's political ideology was a desire to locate political principles on which to erect society. Accepting that the revolutionaries' failures had damaged such an approach, Constant nonetheless associated himself with the revolution and rejected the Burkean view that, in order to function, a polity could only evolve organically and could not be created or formed *ex nihilo*.

Constant prized two principles above all others. First, governments must reflect the "general will" of their populace but, heeding the lesson of the Terror, they must also restrain themselves and never challenge the basic liberties of the individual. According to Fontana, Constant also believed that revolutions thus were good because they reasserted the principle that all authority sprang from civil society. Constant also insisted that the historical characteristics of a society would limit its political options. Advocate of both the general will and human rights, Constant here manifested another ambivalence by justifying revolution and arguing for limited change. Two other problems occupied his energies. His theories

called for political equality but, like the most revolutionary people of his day (excepting Gracchus Babeuf), he allowed social and economic inequalities. Although Constant hoped to moderate such differences, he depended on education and economic development to do so. He also worried that public opinion, the representation of civil society's views, would become tyrannical. To prevent such an occurrence, he prescribed open and uncontrolled discussion.

Fontana ably delineates a vision of Constant's politics that clearly is not simply reactionary. Finding some good in the revolution gives just one example of Constant's complex ideological stance. But the author has not explained how her interpretation advances knowledge more generally. In the wake of communism's virtual collapse, her formulation of Constant's ideas could suggest to contemporary conservatives that the liberalism they claim to follow may be quite moderate, and to leftists that this ideology may be a source of ideas about how to rescue their revolutionary tradition. It does not, then, appear far-fetched to ascribe to her a goal that Holmes also adopted—an unabashed attempt to discover a liberalism more vibrant than its stereotype, which might be useful for contemporary politics. Furthermore, her frequent identification with Constant's ideas indicates that she believes them to have resonance today.

JACK R. CENSER
George Mason University

JEAN-PAUL MASSALOUX, *La régie de l'enregistrement et des domaines aux XVIII^e et XIX^e siècles: Etude historique*. (Hautes études médiévales et modernes, number 64.) Geneva: Droz. 1989. Pp. xx, 414.

The administration that is the subject of this book was a distinctive and exceptionally long-lived institution. It charged fees on an extensive range of legal documents, recording for its own use a brief summary of their contents. It also collected the stamp taxes, various other fees, and payments from users of the royal and later the national domain. Revived under Louis XIV in his ceaseless quest for sources of revenue, reorganized by the *fermiers généraux* in 1727, it finally disappeared in 1948 in a merger with the administrations of direct and indirect taxes.

Jean-Paul Massaloux had a distinguished career in the *Enregistrement et Domaines*, rising to the position of regional director. His working familiarity with the administration gives him an unusually secure background for this book. The mastery of facts and the precise commentaries on legislative texts that were characteristic of the *Enregistrement et Domaines* administration are employed here to good effect. Theories of the development of bureaucracy, perspectives that emphasize surveillance in the interests of state-imposed order, as well as questions about the social

origins and connections of the administrators are left to be supplied by the reader.

The early origins of the royal domain and of the registration of documents form a complex and difficult story that is merely sketched. Most of the book is devoted to the period between 1690 and 1820. A brief final chapter characterizes the administration as a stable element amid the swirl of political and economic changes in the nineteenth century. This stability can be seen, for example, in the fact that registration fees amounted to 11 percent of total receipts by the national treasury in 1799, and 11.9 percent in 1883.

Institutional survival can offer suggestions concerning broader topics. The circumstances that kept the *Enregistrement* going included the French legal practice of relying on notarized documentation for important transactions, the generally modest amounts of the registration taxes, the close supervision of some of the *fermiers généraux*, and the social standing and secure incomes of the collectors, some of whom were able bureaucrats. In describing the role of the *fermiers généraux*, the fees and taxes collected, the methods of collection, and the recruitment, work, and remuneration of the employees, Massaloux provides valuable information on many points. The summaries of documents and the alphabetical tables that the administration produced are still used by many researchers, who will be especially interested in this book.

Although well administered, these taxes were intrusive, difficult to evade, and unpopular. Doubtless Massaloux would not be surprised to learn that in 1789 the fees for the *contrôle des actes* and *insinuation* were the object of complaints and protests in more electoral circumscriptions than any other indirect tax (see Gilbert Shapiro, "Les demandes les plus répandues dans les cahiers de doléances," *L'image de la Révolution Française* [3 vols., 1989], 1: 7–14). All the more remarkable, then, is the reestablishment of these fees. The prerevolutionary administration was abolished in May 1791, and 57 percent of the former regional directors immediately became departmental directors in the new administration. The successive revolutionary regimes relied on it, after August 1791, to manage the vast business of the nationalized property that had belonged to ecclesiastical institutions, the royal domain, and émigrés.

After 1796, newly established taxes were assigned to it, as the only fiscal agency actually organized and functioning. Massaloux gives an enlightening account of the celebrated law of 22 *frimaire* Year VII (December 12, 1798), which was to serve as the legislative basis for the *Enregistrement et Domaines* for 150 years.

Biographical details concerning many members of the administration are found at various places in the volume. Useful appendices list the 138 directors by *généralité* between 1761 and 1791, and the 461 directors by *département* in France between 1791 and

1831, with biographical information about some of them.

PHILIP DAWSON
Brooklyn College
Graduate School and University Center,
City University of New York

EVELYN BERNETTE ACKERMAN. *Health Care in the Parisian Countryside, 1800–1914*. New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xvi, 245. \$45.00.

JACK D. ELLIS. *The Physician-Legislators of France: Medicine and Politics in the Early Third Republic, 1870–1914*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1990. Pp. xii, 305. \$49.50.

These two books present distinctive perspectives on the encounter between medicine (primarily the profession, but also the academic discipline) and society in nineteenth-century France. Evelyn Bernette Ackerman studies “the management and understanding of disease” or “the community of the sick and their healers” (p. 1) in the Department of Seine-et-Oise, adjacent to Paris, between 1800 and 1914. She attempts to emulate the French local studies, which have illuminated social history “from below.” Jack D. Ellis has written a prosopographical study of the surprisingly large group of physicians who sat in the Chamber of Deputies and the Senate of the Third Republic between 1871 and 1914. Although obviously an elite of sorts, these doctors were at least legislatively engaged with the health and welfare of all their fellow citizens.

Ackerman begins with two scene-setting chapters on the region and attitudes about health, and the size and structure of the licensed medical corps, including doctors, health officers (trained in secondary schools and licensed to practice only in one department), and midwives. She then proceeds more or less chronologically through the major medical events of the nineteenth century: early efforts begun under Napoleon to establish “epidemic doctors” in the country and to introduce Jennerian vaccination against smallpox; the cholera visitations in 1832, 1849, and after; the impact of the germ revolution on the medical corps and population of Seine-et-Oise; and the state’s increasing involvement in health care beginning in the Second Empire. Her final chapter is an institutional study of the small hospital of Mantes-la-Jolie over the course of the century.

Ackerman’s ambition—to describe the encounter between ordinary people and their healers—is circumscribed by her sources, which tend to be generated by medical professionals and their administrative allies. These include reports by epidemic doctors, papers of the department’s hygiene council, hospital registers, and judicial records on illegal practice. The reader has a sense of an undocumented bottom of the iceberg, which the author often tries to discover

through speculation based on other studies of the popular mentality in France in different periods.

Ackerman’s ambition also leads her at times to emphasize the more rural and backward parts of what was, after all, a fairly urbanized and progressive department. Thus, for instance, the hospital she chooses to study is only the eighth largest in Seine-et-Oise, one-sixth the size of the one in Versailles. As Ellis’s book and some of Ackerman’s own evidence show, the department of Seine-et-Oise was one of the most conscientious in France in establishing a system of epidemic doctors and promoting smallpox vaccination and had an unusually active hygiene council. Finally, however, as Ackerman does demonstrate, it was not administrative efforts but the overwhelming evidence of the success of rabies inoculation and the diphtheria antitoxin that overcame popular alienation from professional medicine in the 1880s and 1890s.

Ellis’s book begins with a puzzling circumstance: the unusually high proportion of medical doctors in the parliaments of the early Third Republic (1871–1914). Some 358 physicians were elected as deputies or senators during this period. They ranged from 10 to 12 percent of the membership of most legislatures, a proportion exceeded only by jurists. The proportion of all French doctors serving in parliament in 1891 was 0.57 percent, compared to 0.42 percent of all jurists (*avocats*, *avoués*, magistrates, and notaries) and even smaller percentages for other professions.

Ellis attempts to understand the unusual attraction of parliament for the physicians of the early Third Republic by asking a series of behaviorist questions: Who were the physician-legislators? Why did they enter politics? Like Lewis Namier and more recent political scientists (Ellis cites Oliver H. Woshinsky), Ellis contends that people’s actions in politics follow from their reasons for entering politics. Ultimately, he is interested in the distinctive role played by the physician-legislators in parliament. Thus, the book breaks neatly into two parts, the first on the doctors’ pre-parliamentary careers (social backgrounds, early medical careers, local political careers) and the second on their parliamentary roles (parliamentary career patterns and legislative roles with respect to medical reform, health issues, and social issues).

The research basis of this book is a computerized study of the 358 physician-legislators of the early Third Republic. The sources for this study include biographical dictionaries, academic records, medical dissertations, campaign literature, parliamentary debates, and the medical press. In the interpretation of these sources Ellis demonstrates a thorough knowledge and understanding of medical, political, and parliamentary careers and major public issues of the period.

The picture that emerges of the physician-legislators is not an undifferentiated one but is characterized by a predominance of rural and small-town practitioners from the south, southwest, and other

less-industrialized areas, who tended to be on the left of the political spectrum (radical rather than socialist), especially in the early part of the period. Perhaps their most consistent political position was their anticlericalism in the 1880s. Their sympathy for the poor, developed in their hospital training and private practice, was more pragmatic than ideological and was most manifest in their activity on behalf of health legislation (except where it conflicted with local economic interests, such as the wine industry). They were not leaders in more general social and industrial legislation, nor were they simple agents of their own professional interests.

Both of these studies point to a shift in the relationship of medicine and society in France in the late nineteenth and early twentieth centuries. Ackerman cautiously discerns an increased willingness on the part of the rural populace to call on professional medical services, although not to the exclusion of folk remedies. She attributes this change to economic improvements and the accompanying willingness to believe in the possibility of improvement in other realms as well, increased access to professional medicine through state support, and especially the demonstrated successes of professional medicine as a result of the germ revolution.

Ironically, at the same time that the populace is thus showing more faith in professional medicine, Ellis sees the doctors in the parliaments of the Third Republic edging away from their earlier progressive social program into a more conservative defense of their professional status and autonomy. This shift begins to emerge in the 1890s, with the rise of medical syndicates and labor unrest, and becomes distinctly manifest after World War I, when a somewhat reduced corps of physician-legislators lost the distinctly leftist orientation that characterized their predecessors in the era of Léon-Michel Gambetta and Jules Ferry.

GEORGE D. SUSSMAN
Delmar, New York

DOMINIQUE DESSERTINE. *La Société Lyonnaise pour le Sauvetage de l'Enfance (1890–1960): Face à l'enfance en danger, un siècle d'expérience de l'internat et du placement familial*. (ETHISS.) Toulouse: Erès. 1990. Pp. 218. 142 fr.

La Société Lyonnaise pour le Sauvetage de l'Enfance was one of many philanthropic organizations that arose in France at the end of the nineteenth century designed for the protection, education, and incarceration of children. A study of any of these societies could illustrate the fluidity of the relationship between private institutions and public social policy during the Third Republic; it could also serve to illustrate changing attitudes toward children, especially those mistreated or delinquent. Dominique Dessertine leaves these larger issues virtually unexplored.

Basing her book almost solely on the archival records of this one society, Dessertine presents a traditional history of a particular association serving a few hundred boys each year and governed by just a few personalities during its existence. The only noticeable shift is from anti-clericalism to social Catholicism.

Aside from an excellent introduction and first chapter—fourteen pages that mention some of the relevant legislation, motives for legislative changes, and proposed solutions to social problems—there is little in this book to commend it to nonspecialists. Dessertine does not sufficiently relate this organization to others in different cities, to the development of adolescent reform schools, to changing definitions of “difficult” children, or to the different legislation passed since its founding. Philanthropists founded this society in Lyon initially to implement the national legislation of 1889 on “morally abandoned” children, those children whom the state took away from their parents to protect them from parental mistreatment, neglect, and crime. Between the passage of this legislation and 1960 (when Dessertine's study ends), the French state made numerous other attempts to deal with juvenile delinquents, limit paternal authority, and establish alternatives to putatively derelict parents, in the interest of protecting the children and society. Legislators and social reformers debated familial placement of “difficult” children, the need for reform schools to vocationally train children, and the necessity of juvenile detention centers for “vicious” children. But Dessertine fails to link adequately the changes in this institution with these broader debates or legislative initiatives.

Designed for the education of difficult and vicious children, la Société Lyonnaise was both a reform school and an agency that placed children with families, but over the course of the decades familial placement became increasingly rare. Dessertine provides a detailed description of the interior design and the severe daily regimen of silence and discipline at Sacuny, the institution used as a detention center for the vocational training of those children too difficult to be placed with a family. By the 1920s the institution became less a center for children confined by the Court Tribunals and more one in which parents placed their unruly or mentally ill children. After 1925 children interned there by the 1889 law were rare and those incarcerated by their parents constituted 40 percent. The average length of stay was three years—longer for children confined by the courts, shorter for those placed by their parents. For eight years after the 1921 law on the prostitution of minors, this society accepted girls accused of prostitution, but it failed to develop a system for them. Dessertine affirms that throughout the period covered the patrons and personnel of the Society believed that their organization was one of crime prevention, and that society and the children could best be protected if children were removed from their

corrupting environment and taught a trade and discipline in an approved family or institution.

Despite this book's narrow and unanalytical focus, some historians might find this densely detailed work useful as a case study. These include historians of Lyon looking for information on the philanthropic activities of some of the leading men of that city, specialists in the history of philanthropy, and scholars exploring institutional treatment of juvenile delinquents. Readers will have to supply their own contextualization.

RACHEL G. FUCHS
Arizona State University

ROGER BOURDERON and IVAN AVAKOUMOVITCH. *Détruire le PCF: Archives de l'Etat français et de l'occupant hitlérien 1940-1944*. Paris: Messidor/Editions sociales. 1988. Pp. 274. 120 fr.

This short volume, by historians either part of or sympathetic to the Parti Communiste Français (PCF), purports to be an advance over previous accounts of the wartime activities of the party in that it is based on German occupation documents, internal party sources, and archives of the Vichy regime. In fact one finds little that is new or unexpected, and the preponderance of notes from the French national archives indicates that the German sources were not much used and internal PCF sources even less. Not surprisingly, Roger Bourderon and Ivan Avakoumovitch show that both Germans and the Vichy state attributed the bulk of resistance activity in France to Communists and Jews (terms that the Nazis, more than Vichy, used interchangeably). Also not surprisingly, the authors use this evidence to demonstrate that the Communists spearheaded the French Resistance and that consequently both Vichy and the Gestapo attempted to destroy the PCF. At the risk of belaboring the obvious, everyone knows that Communists played a disproportionate role in the French Resistance, as did Jews for that matter; these become no less historical truths because they were loudly trumpeted by the Germans. In fact the authors go to the trouble of disclaiming any particular truth to the charges in the case of the Jews, but accept them at face value in the case of the PCF.

The thematic organization of the book allows the authors to gloss over the differences in the Communist party line before and after the German invasion of the Soviet Union on June 22, 1941. Individual Communists may have resisted before that date; the party as such did not. This has been amply demonstrated elsewhere despite the authors' claims (see in particular the two volumes edited by Jean-Pierre Azéma, Antoine Prost, and Jean-Pierre Rioux, *Le Parti Communiste français des années sombres, 1938-1941* [1986] and *Les Communistes Français de Munich à Chateaubriant, 1938-1941* [1987]). The authors have little new light to shed on the murkier aspects of party

policy during this period. They adhere to the standard party line that the proposal to the Germans to allow the legal publication of the French Communist press was taken on the individual initiative of Maurice Tréand and Jean Catelas without the authorization of the party leadership, something nobody outside the PCF believes today. The controversy over the betrayal of the Manouchian group of Communist émigré terrorists, most of whom were Jews, to the Gestapo in 1943 is similarly ignored by the authors; one finds no willingness here to deal with the charges by former leaders of that group that the party itself betrayed them.

One may readily grant that the policies of Vichy and the German occupiers with regard to destroying the PCF were "continuous, coherent, and complementary," (p. 244) and perhaps the extent of their cooperation is documented here in a way that it has not been elsewhere. But were the occupiers, for all that, really a "formidable social rampart" against Communist-led revolution for the French ruling class (p. 244)? On the contrary, it would seem more accurate to say that the Communists were no real threat to anybody in the 1930s but emerged as a genuine menace to the regime after 1945, in large part thanks to Vichy and the German occupation.

IRWIN M. WALL
University of California,
Riverside

HERRICK CHAPMAN. *State Capitalism and Working-Class Radicalism in the French Aircraft Industry*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xvii, 412.

Readers should not be put off by this book's somewhat ambiguous title or by the impression that this is a narrowly focused industrial history. By concentrating on the aircraft industry and demonstrating how in 1936 government, employers, and a Communist-dominated labor movement combined to transform industrial relations in this sector of the economy, Herrick Chapman succeeds in a broader goal. He explains the emergence in France after 1945 of a state-managed capitalist economy and a mass-based labor movement dominated by the French Communist Party. The roots of *dirigisme* on the one hand and of working-class radicalism on the other lie deep in the past, the author reminds us, but the contemporary pattern essentially emerged from the turbulent years between 1936 and 1948.

Cutting across traditional "historiographical" boundaries, Chapman covers the depression, the Popular Front, rearmament, military defeat, the Nazi occupation, liberation, and the formative years of the Fourth Republic. He calls attention to the two critical "cycles" of "innovation and retrenchment," the first in 1936-38, the second in 1945-50. Critics may complain that he telescopes the two periods somewhat

and overemphasizes the "leap on a grand scale" in the earlier period to direct forms of state control, but Chapman nevertheless aptly describes the profound changes brought by the Popular Front government when workers won collective bargaining rights, paid vacations, limitations on working hours, and shop representation. No one could have predicted the abrupt change from liberal orthodoxy as well as the government's enhanced role in industrial relations (including, for a time, a form of compulsory labor arbitration). Once again in 1945, with liberation, even more extensive reforms, including broad nationalization measures, were adopted. What understandably intrigues the author is that at the end of each period labor experienced severe setbacks yet many of the innovative reforms were retained, government controls remained entrenched, and employers learned to live with the new pattern.

In the 1920s and 1930s small, inefficient family firms in the aircraft industry could not achieve the concentration needed for expansion once rearmament became imperative. Nationalization was a key to rationalization and might have come without the Popular Front, but the form of partial nationalization it took under Léon Blum (and his air minister, Pierre Cot) and the increased role it gave to labor was clearly the product of the social explosion of 1936. To be sure, the aircraft industry was atypical because defense contracts made the state's role more prominent than elsewhere in the economy and the industry had a highly skilled and well-paid labor force, but every major shift in politics had its repercussions in the industry, thus making it an admirable focus for a broader inquiry into labor relations.

The author correctly notes that the years 1934–50 left "powerful memories and durable myths" and helped shape the contemporary industrial scene (p. 316). Perhaps future historians will see a third cycle of innovation and retrenchment in the François Mitterrand experience, which this book by its nature can only touch on. The supersession of the French Communist Party by a social-democratic Socialist Party has finally made it possible for France to move toward the social democracy that most West European countries had come to know in the early post-1945 years and to end the "unusually contentious" style of labor relations graphically described in these pages.

Chapman has done a remarkable job of primary research and lucid synthesis, tapping archival sources, utilizing the most dependable secondary literature, conducting interviews, and succeeding in an exemplary way in using a monographic study of a single industry to illuminate a broader pattern of historic change in industrial relations.

JOEL COLTON
Duke University

MARÍA MILAGROS CÁRCEL ORTÍ, editor. *Relaciones sobre el estado de las diócesis valencianas*. Volume 1, Orihuela;

volume 2, Valencia; volume 3, Segorbe. Foreword by VICENTE CÁRCEL ORTÍ. (Col·lecció fonaments, number 2.) Valencia: Generalitat Valenciana Conselleria de Cultura, Educació i Ciència. 1989. Pp. 693; 698–1433; 1441–1958.

María Milagros Cárcel Ortí's volumes provide historians with a useful source for the study of the history of the Spanish church between 1589 and 1900. The documents edited here (found in the *Archivo Segreto Vaticano*) are the reports that the archbishops of Valencia and the bishops of Segorbe and Orihuela made to the pope on the material and spiritual situation of their respective dioceses. These reports were an integral part of the *ad limina* visit to Rome, which the bishops of the church were obliged to undertake every five years and which also entailed venerating the tombs of the apostles Peter and Paul. Pope Sixtus V (1585–90) was responsible for institutionalizing the *ad limina* visit as part of the effort of the papacy after the Council of Trent to oversee more effectively the pastoral activities of the bishops. Although the bishops themselves did not begin to travel to Rome until the mid-nineteenth century, they sent in their stead representatives who presented the reports to the Vatican.

Helpful introductory chapters provide a detailed account of the development of papal *ad limina* legislation and a fine diplomatic analysis of the documentation that also helps explain how the papal bureaucracy functioned. Of perhaps greater interest to historians is the editor's use of the *ad limina* reports for a demographic study of Valencian parishes in the seventeenth and eighteenth centuries. Given the lack of parish registers for many localities, these reports acquire particular importance and provide data, for instance, on the number of places left depopulated in the wake of the Morisco expulsion (1609–10) or the toll taken by the devastating epidemics of 1647–52.

The edition of the reports and of the Vatican's responses to them is of high quality. Although some of the reports, as the editor points out, are too brief to be of much use to historians, while others are merely copies of previous reports, there is no doubt that this collection contains material of considerable interest. For example, the reports discuss the failure of the Valencian church to educate the Moriscos in the Catholic faith, a failure explained in part by the fact that the clergy often did not reside in Morisco parishes. The reports also reveal the difficult process of reforming the Spanish church in accordance with the dictates of the Council of Trent, particularly with regard to the foundation of diocesan seminaries for the education of the clergy. On the positive side, the bishops describe the work of caritative foundations in administering to the needs of the poor, many of whom were homeless. Their listing of the various religious confraternities in their dioceses provide insights into the evolution of popular devotional practices. Even political events receive attention in the

reports; for instance, the bishops note how the War of the Spanish Succession was wreaking havoc in their dioceses and making their labor more arduous. In their detailed discussion of the revenue of parish churches and of the number of clergy, convents, parishioners, schools, and caritative institutions, the *ad limina* reports tell us much about the structure of the Catholic church at the diocesan and parish level and about the relative abundance of material and manpower resources needed by the church to carry on its pastoral work. This documentation is an important complement to those sources, such as Inquisition records, used by historians to analyze local religious beliefs and practices. A more detailed knowledge of the church's material limitations at the local level will further our understanding of the impediments that had to be overcome in the process of ecclesiastical reform. We are fortunate that Cárcel Ortí has provided us with the sources from which to acquire such knowledge.

MARK D. MEYERSON
University of Notre Dame

BURNETT BOLLOTEN. *The Spanish Civil War: Revolution and Counterrevolution*. Chapel Hill: University of North Carolina Press. 1991. Pp. xxxii, 1074. \$59.95

This monumental study is the final, and impressive, testament to a life's work. It also has a long and interesting history of its own. Burnett Bolloten was a journalist who chose to begin a holiday in Spain in mid-July 1936. He remained in Spain during the Civil War and then spent much of the rest of his life, first in Mexico and then in California, gathering materials and writing the history of that epochal event. The first version, or rather the earliest ancestor, of this book was published in 1961 under the title *The Grand Camouflage*. Its critique of the Communists made it attractive to the regime of Francisco Franco, and an unauthorized Spanish edition was published almost immediately. An authorized Spanish-language edition was published in Mexico in 1962. *The Spanish Revolution*, a much-expanded version of the 1961 volume and the current book's immediate predecessor, appeared in 1979.

The present book is the longest and most complete of the series. In one important respect it is a significant improvement: unlike the other two volumes, which dealt with only the first ten months of the Civil War, this one deals with the entire conflict. This volume shares both the virtues and the shortcomings of the earlier books. In all of its manifestations Bolloten's work stands out for its remarkable erudition. The number and range of sources that he brings to bear is unsurpassed. Indeed, Bolloten built his own collection of sources on the Civil War, now housed in the Hoover Institution, comprising one of the most important archives on this topic in the world.

Unfortunately, Bolloten's histories of the Spanish

Civil War have consistently suffered from a flaw, one serious enough to be considered fatal. Bolloten has held tight to a single interpretative line; that the story of the Spanish Civil War is one of how, in the interests of Soviet foreign policy, the Spanish Communist Party and the agents of the Comintern killed the social revolution that had taken place during the early months of the war. There is certainly much to criticize in the conduct of the Communists, but such a critique must be firmly located within the context of events, in this case the fact that the Republic was fighting a war against the Nationalists and their foreign, fascist supporters. Can there be any doubt that the overriding consideration for all those who opposed Franco was winning the war and that the conduct of all groups in the Republican camp must be evaluated in this light? Did the Communists' rivals have a more effective way of fighting Franco?

In addition, Bolloten's obsession with the Communists leads him to tell a story in which only the Communists act and the other political forces are passive or, at best, react—always ineffectively—to Communist initiatives.

Despite these problems, Bolloten's work is and will long remain one of a handful of indispensable texts on one of the most intensively studied events in European history. That is a legacy that very few historians can claim.

ADRIAN SHUBERT
York University

WAYNE PH. TE BRAKE. *Regents and Rebels: The Revolutionary World of an Eighteenth-Century Dutch City*. (Studies in Social Discontinuity.) Cambridge, Mass.: Basil Blackwell. 1989. Pp. xix, 213. \$39.95.

Flourishing once again, comparative history remains relatively isolated from the recent proliferation of local and regional studies. Senior scholars speculate on the similarities linking various national political movements while graduate students are dispatched to pursue research in small villages, market towns, and regional centers throughout France, England, and Germany. Wayne Ph. Te Brake successfully brings together these two approaches in his analysis of the Patriot Revolution of 1781–87. He sets his study of the "regents and rebels" of Deventer, a small city in the eastern Dutch province of Overijssel, within the comparative framework of Charles Tilly's theories of economic development and revolution and R. R. Palmer's *Age of the Democratic Revolution* (2 vols., 1959–64).

Artisans and shopkeepers dominate Te Brake's story as they did Deventer at the beginning of the Patriot Revolution. Easily mobilized through the guilds, they formed the core of the petitions from 1782 and 1783 protesting the rule of the Prince of Orange. Individuals outside the corporate structure, mostly lawyers and entrepreneurs, also signed the

petitions. This broad-based coalition, according to Te Brake, was typical of the early stages of eighteenth-century revolution.

When the victorious Patriots convened in 1786 to draft a new constitution, the revolutionary coalition splintered. The framers of the constitution emphasized the equality of individuals and explicitly recognized the rights of Catholics. According to Te Brake, these lawyers envisioned a "brave new world of democratic representation" (p. 122). Te Brake's close study of the signatures on the Patriots' petition of 1787 shows a dramatic shift away from the local market-oriented butchers and bakers, the guild leaders who had led the initial opposition movement, to exporting entrepreneurs, such as the specialty *koek bakkers* and brewers and their unskilled workers. The artisans and shopkeepers made their peace with the Prince, hoping to protect their corporate privileges.

Te Brake convincingly compares the revolutionary politics of the corporate groups and individuals of Deventer with Lynn Hunt's French provincial revolutionaries. His tables and graphs are unfortunately less useful for comparative purposes than the suggestive explanations of his text.

My major criticism of Te Brake is that he suffers, as do most historians of small countries, from what Simon Schama once called the "presentiment of the obscurity of the topic" (*Patriots and Liberators* [1977]). To emphasize the importance of provincial Dutch politics, he jumps from specific analyses of revolutionary institutions to general conclusions such as "the private aristocratic politics of the past had been shattered and the foundations had been laid for the public, participatory politics of the future" (p. 168). The Deventer guild leaders who were motivated only by "matters of transparent self-interest and traditional prejudice rather than philosophical disagreement," according to Te Brake, are in fact no less "political" than the bread rioters and assembly delegates championed by French and English historians such as Olwen Hufton, Dominique Godineau, John Bohstedt, and E. P. Thompson (p. 113). It is precisely because his revolutionaries plotted and schemed well beyond the influence of Paris, London, or even Amsterdam that Te Brake's comparative perspective on the age of revolution is so provocative and persuasive.

JANET L. POLASKY
University of New Hampshire

JOHAN SÖDERBERG *et al.* *A Stagnating Metropolis: The Economy and Demography of Stockholm, 1750–1850*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 13.) New York: Cambridge University Press. 1991. Pp. xii, 234. \$49.50.

This interesting and informative volume paints a vivid, if mainly statistical, picture of economic, demographic, and social conditions in Stockholm between

roughly 1750 and 1850. It is, however, not a particularly pretty picture. The period between the end of the empire (ca. 1720) and the onset of rapid industrialization (ca. 1850) was one of stagnation for Stockholm. The declining role of the state, and state-supported "manufactures" in particular, caused the city to lag behind the country, which itself was no dynamo of change. Among European cities, Stockholm's population rank dropped from thirtieth to fifty-fourth (pp. 11–12).

The remarkably rich supply of historical data available for Sweden, both at the national and at the local level, have allowed these, like so many other, Swedish economic and social historians to build their narrative on a solid statistical base. Levels and trends in nominal and real incomes, in income and wealth inequality, in age and sex-specific mortality, in illegitimacy, and in age and sex-specific poverty rates are only some of the enlightening statistical series developed for the city.

The most striking, even startling, result that emerges from these data is that Stockholm was truly a death trap for men. By any standard, adult male death rates in Stockholm (especially for those aged 25–49) were incredibly high. This was the case relative to death rates for women in Stockholm, for men in the rest of Sweden, and for men in other European cities. While women and children also died at high rates in Stockholm, it was the unusually high death rates of men, supposedly in their prime, that made Stockholm something unique.

The unfortunate situation of working-class and working-age men in Stockholm is confirmed by the data on poverty. Up to age fifty, poverty rates for men were substantially higher than those for women. In old age, the situation was reversed, but by then there were very few surviving working-class men (p. 57). The economic reality underlying these poverty rates seems to be that the declining manufacturing activity in the city made it particularly difficult for adult men to find permanent and secure, even if poorly paid, employment. Working-class women, who were likely to be employed as servants on annual contracts, were in a somewhat better, or perhaps less miserable, situation. Furthermore, those "informal" (unregulated) economic activities that primarily employed women were less likely to arouse the ire of the authorities than were those dominated by men (p. 54).

One of the intriguing consequences of the relatively good economic situation of working-class women appears to have been a low rate of nuptiality and a high-rate of illegitimacy. The point, of course, is that these rates were not so much the result of unusually high rates of promiscuity as of the prevalence of more-or-less permanent, but unconsecrated, unions (also known as "Stockholm" marriages). Given their good situation in the labor market, as well as the patriarchal nature of family law, working-class

women had little incentive or need to formalize their unions.

In view of the fact that the authors are not native English speakers, the text is remarkably good. At the same time, however, enough Swedish-sounding syntax remains to make it difficult to believe that it has been professionally edited. Frankly, I would have expected more from the Cambridge University Press.

LARS G. SANDBERG
Ohio State University,
Columbus

STEVEN DUNCAN HUXLEY. *Constitutionalist Insurgency in Finland: Finnish "Passive Resistance" against Russification as a Case of Nonmilitary Struggle in the European Resistance Tradition*. (Studia Historica, number 38.) Helsinki: Suomen Historiallinen Seura. 1990. Pp. xiii, 284.

Steven Duncan Huxley's study of constitutionalist insurgency in Finland offers an intriguing, but not entirely convincing, interpretation of the Russification era at the start of the twentieth century. The author seeks to place the Finnish case into a broader international and theoretical framework; nearly one-quarter of the book deals with a critique and analysis of the theory and practice of passive resistance in European history. The documentation is impressive, including a strong familiarity with the literature of nonviolent resistance and the use of rare primary sources.

Huxley characterizes the dominant feature of the years 1898–1905 as "active passive resistance" (p. 175) or "nonmilitary guerrilla warfare" (p. 176). The roots of constitutionalist resistance are to be found, first of all, in an elitist Finnish nationalism that developed in the context of the Diet beginning in the 1860s. Particularly significant was J. V. Snellman's emphasis on cultural advance as the key to national survival. The other major element the author sees in Finnish political culture before 1898 is a preoccupation with justice and legal arguments in defense of Finland's political autonomy. Finnish political thinkers were also aware of the European passive resistance tradition. The most important model for Finland was the Hungarian resistance to Austrian rule after 1850 that successfully culminated in the Compromise of 1867.

Passive resistance was possible in Finland in the years before 1905, Huxley argues, because Russification was not an all-out but rather a limited assault on Finnish institutions. One writer summarized the resistance program in 1900 as "noncooperation, disobedience, nonrecognition" (p. 169), and Huxley credits the movement with a number of successes, including various boycotts and the establishment of a thriving underground press. Perhaps most significant, according to the author, was the development of an ideologically sophisticated nonviolent resistance literature. Nevertheless, Huxley also finds that the Finnish

resistance weakened by 1903, worn out by constant battles with the rival compliance faction and an inability to mobilize a permanent mass basis for the movement. In the end it was the assassinations of Nikolai Bobrikov and Viacheslav Plehve in 1904 and the Revolution of 1905 that saved the constitutionalist cause in Finland. Reflecting its particularism, the Finnish passive resistance maintained an ambivalent attitude toward the all-empire liberation movement. After 1908, as tsarist attacks on Finland were renewed, passive resistance was no longer a viable option because of the deep divisions in Finnish society. The author correctly concludes that the Finnish case in the period from 1898 to 1905 was sufficiently unique to preclude drawing any wider conclusions from this example.

Huxley's subject is well known in Finnish historiography; what he offers is a reinterpretation and a fresh perspective. In this effort the author achieves considerable success, although the European frame of reference is not as clearly related to the Finnish experience as it could be. Most importantly with regard to the Finnish situation, the book does not bring us closer to an answer to certain key questions such as the depth and breadth of Finnish nationalism in these years or the relationship of the elites and the masses of the population. We learn a great deal about a few leading activists and political thinkers, but the "constitutionalist front," as the author terms it, remains vague and undefined. Nevertheless, Huxley's efforts should be applauded for broadening our perspective on this crucial era.

TOIVO U. RAUN
Indiana University,
Bloomington

MARY LINDEMANN. *Patriots and Paupers: Hamburg, 1712–1830*. New York: Oxford University Press. 1990. Pp. vii, 339. \$39.95.

Hamburg has become one of the largest cities of Europe since the eighteenth century. The history of its commerce and its social problems has attracted historians in many countries. Mary Lindemann's monograph is one of the first to appear in English. An exemplary work, it may serve as a model for the general student of Western welfare institutions. Her lively style and penetrating analyses get at the heart of Hamburg's patriotic reformism. Her chief interest is in the welfare reforms of 1788, which were designed to provide loans, retraining, and a subsistence income for Hamburg's neediest families. The new welfare system grew out of the traditional Lutheran parish alms fund. The city council reorganized the decentralized parish administration and concentrated the relief administration in the Public Assistance Board, directed by some members of the city council and by the parish deacons. As many able-bodied individuals

as was feasible were to be restored to full employment.

Why did this public-spirited reform take place when it did and why did it seem to collapse in an atmosphere of indifference after 1815? Lindemann looks to the influence of the Enlightenment in Hamburg for an explanation. Most important was the founding of the Patriotic Society in 1765. When the 1788 reform began, the depression precipitated by the Seven Years' War had already ended. Lindemann argues that there was an impelling need for more rational administration. That the reform succeeded during the 1790s owed much to the enlightened bourgeoisie of Hamburg. The war boom of that decade must certainly have helped. The author also shares the traditional view of Hamburg historians that the French occupation of 1806 brought about a general collapse. The Hamburgers were in favor of laissez-faire policies before the French Revolution. Did they become protectionist after 1815 and does this explain the budget cuts in the welfare program?

If the eighteenth-century government was dominated by an elite group of merchants and intellectuals, why did the nineteenth century seem to produce even more oligarchy? Lindemann's statistical compilations again provide new material. Only 8.7 percent of Hamburg's inhabitants were citizens in 1875, a decline from the 20 percent figure of 1759 (p. 212). Did this result from structural changes associated with the industrial revolution generally? Did the number of paupers also decline as industrial recovery took place?

Lindemann has presented a masterful study of an important epoch of Hamburg's history. Material in the Hamburg State Archives and the Commerce Library has been skillfully analyzed. The author combines the history of industry, commerce, and welfare by using the methodology of the *Annales* school. She likes the rational spirit of the philanthropist Caspar Voght, one of the leading reformers, who tried to rationalize the idea of charity. The hardened attitude that emerged after 1815 was the result of changed times. The world of welfare and pauperism had been deeply troubled by war and inflation. Would the Enlightenment's conception of relief survive in an age of progressive industrialization? Lindemann is certain that the old spirit had disappeared and that a new age of frugality had replaced it.

HELEN LIEBEL-WECKOWICZ
University of Alberta

PAUL LAWRENCE ROSE. *Revolutionary Antisemitism in Germany from Kant to Wagner*. Princeton: Princeton University Press. 1990. Pp. xviii, 389. \$29.95.

Paul Lawrence Rose's theme is the emergence of anti-Semitism in Germany within the context of "revolutionary" thinking, by which he means, very loosely, anyone engaged in or sympathetic to revolu-

tions, even anyone with a "revolutionary temperament, whether in philosophy or in politics or in literature" (pp. 16–17, n. 18). He begins by explaining his theory of the emergence and the essentially interconnected nature of German revolutionary and anti-Semitic thinking. He traces the progress of this hydra from Christian Wilhelm von Dohm and Johann Gottlieb Fichte to young Germans like Ludwig Börne, Heinrich Heine, Karl Gutzkow, and Heinrich Laube. He devotes substantial time to David Friedrich Strauss, Ludwig Feuerbach, Georg Friedrich Daumer, Friedrich Wilhelm Ghillany, Bruno Bauer, Wilhelm Marr, Karl Marx, and Moses Hess. He concludes with the final juncture of revolution and race in the works of Constantin Frantz and Richard Wagner.

Before criticizing this effort, one which clearly demanded an extraordinary investment of time, I should note that much of the book is well written and based on an impressive amount of research. Rose includes a number of figures, like Karl Gutzkow and Heinrich Laube, who merit more extensive treatment than normally accorded them. The author's argument that anti-Semitism shifted from an "ethnic" to a "racial" idea in the era from 1780 to 1860 is especially useful (pp. 44–50). Additionally the author presents some valuable insights, most importantly that the older "blood libel," the accusation that Jews murdered Christians, modulated into a "money libel," that Jews commercially exploited Christians (p. 315).

Readers who compare Rose's treatment of complex and prolific thinkers like Immanuel Kant and Johann Gottfried von Herder with that found in James Sheehan's recent excellent *German History, 1770–1866* (1989) will wonder if both authors inhabit the same universe. Even when correct in his analysis of the anti-Semitic nature of a particular writer or philosopher, Rose errs in making this the sole or even the most significant factor in evaluating that person's contribution to European culture.

More serious is Rose's tendency to slippery reasoning. That some supporters of Jewish emancipation were anti-Semitic has been long known, but Rose subtly shifts the assertion to one in which all supporters of emancipation were anti-Semites. So, too, Rose moves from noting that some German revolutionaries were anti-Semitic to statements such as the following: "German revolutionaries of all stripes accepted the implicit racial prejudice that there existed a virtually unchanging Jewish—as well as German—national character. German revolutionism was therefore embedded in a pattern of thought that was essentially racial and politically anti-Jewish" (p. 13). In reality many German "revolutionaries" were pro-Jewish rather than anti-Jewish, even using Rose's bizarre definition of "revolutionist."

Of course some Jews were also revolutionaries, among them Börne, Heine, Marx, and Hess. But Rose implies that all Jews who were "revolutionary" or "radical" criticized Judaism out of self-hatred and

contributed to anti-Semitism. Rose repeatedly condemns, as in reference to Heine, any "ambivalent" position on Judaism; ambivalence caused "unintended" results (p. 161). For not supporting immediate and total emancipation out of a "realistic" estimate of his fellow Germans, Rose portrays Gutzkow as anti-Semitic, while Berthold Auerbach's longstanding belief in German acceptance of Jews merits a chapter so that Rose may expose his naiveté. Karl Marx receives only a few pages, but it is enough to allow Rose to assert that "materialism" possessed a "natural propensity" to lead to racism.

But by far the most pervasive generalizations involve Germans. For example: "In Germany, reason and revolution were conscripted into the service of the new secular antisemitism" (p. 55); and "Like so many other German pronouncements of magnanimity toward the Jews, Herder's is really an alibi and an evasion of German responsibility" (p. 100). Indeed, the key to understanding Rose's work appears where he argues that there is such a thing as "national character" (pp. 62–63) that explains why Germans "reacted so hysterically" to Jews in a "spirit of essential bad faith masquerading as concern for humanity" (p. 67). To deny the phenomenon of German national character, Rose tells us, is to "react in a spirit of liberal prejudice which, while . . . morally inspired, is scarcely objective or respectful of historical reality" (p. 67).

Rose states that the only danger in using "national character" in historical analysis is if one only presents its "unfavorable aspects" (p. 68), but, when he writes about Germans, Rose engages in exactly that which he condemns: he replaces the "Eternal Jew" with the "Eternal German."

JAMES F. HARRIS
University of Maryland,
College Park

RUDOLF M. WLASCHEK. *Juden in Böhmen: Beiträge zur Geschichte des europäischen Judentums im 19. und 20. Jahrhundert*. (Veröffentlichungen des Collegium Carolinum, number 66.) Munich: R. Oldenbourg. 1990. Pp. 236. DM 58.

In this book, Rudolf M. Wlaschek, a German from the Sudetenland residing in Germany since the late 1940s, seeks to restore the Jews of his Bohemian homeland to what he perceives to be their rightful place of honor in German culture. Wlaschek is clearly in deep pain about the Holocaust. He is very upset that the Jews of Bohemia, who were overwhelmingly German by language, cultural identity, and loyalty, were thanked for that loyalty with Theresienstadt and Auschwitz. His book, therefore, serves as a tribute to the Jews of Bohemia, chronicling their assimilation into Czech-German culture and their destruction at the hands of the Nazis during World War II. Avowedly polemical, and not a work of original scholar-

ship, this book nevertheless provides a reasonably good summary of the experience of Jews in Bohemia. It is filled with pain and longing for a world that no longer exists.

The first third of Wlaschek's book describes how the Jews in the Czech lands assimilated into German culture as they were released from their medieval disabilities in the late eighteenth and nineteenth centuries. Wlaschek, of course, emphasizes how utterly German the Jews became, affirming their loyalty to all things German even in the face of intense Czech nationalist pressure for them to abandon a German identity for a Czech one in the decades before World War I. Recently, scholars like Hillel Kieval have argued for the growing Czechification of Bohemian Jewry in the late nineteenth century (*The Making of Czech Jewry: National Conflict and Jewish Society in Bohemia, 1870–1918* [1988]), but Wlaschek makes a very convincing case for the persistence of German identity in most Bohemian Jews.

Yet his bias does make him ignore the significant Czechification of Bohemian Jews in the first Czechoslovakian Republic. Whatever Czech identity Wlaschek acknowledges he blames on growing German anti-Semitism in the 1930s, ignoring such important issues as state policy, political and demographic realities, and even Czech anti-Semitism. Indeed, Wlaschek is very negative toward the Czechoslovak state, both for its anti-German policies in the interwar period and for its expulsion of the Germans—including Jews who still considered themselves German—after 1945.

By the same token, Wlaschek underrates the importance of anti-Semitism among the *Sudetendeutsch*. In his conclusion, for example, he argues that Jews and Germans in the Sudetenland always got along well before the 1930s, especially in schools and in the professions, presumably because Jews were such staunch defenders of *Deutschtum*. Wlaschek clearly sees anti-Semitism among Bohemian Germans as a foreign import, from Vienna in the late nineteenth century, and then later from Nazi Germany. Unfortunately, the evidence points to significant anti-Semitism among Bohemian Germans, with the important exception of Prague. Jews may have always defended *Deutschtum*, but they were not always appreciated for it.

Most of this book deals with the Holocaust, and Wlaschek goes into great detail on Nazi policy in the Protectorate of Bohemia and Moravia and on the deportations to Theresienstadt and the death camps. He even provides thirty pages of tables, listing all of the transports, including information on every single Jew deported from Aussig and Karlsbad from 1942 to 1945. The names make a deep impression on the reader; they serve to remind his fellow Germans that they killed human beings.

Wlaschek also devotes several chapters to the fate of the Bohemian Jews lucky enough to emigrate before 1942. It is clear that in their new homes in

America, England, or Palestine (later Israel), these Jews participated in the larger German Jewish émigré community even as they formed special organizations to represent their specifically Czechoslovak interests. These were German Jews, sharing the fate of all German Jews.

This book may not be a pathbreaking work of scholarship, nor even a complete description of all aspects of Bohemian Jewish life, but it movingly accomplishes its author's goals.

MARSHA ROZENBLIT
University of Maryland,
College Park

SETH TAYLOR. *Left-Wing Nietzscheans: The Politics of German Expressionism, 1910–1920*. (Monographien und Texte zur Nietzsche-Forschung, number 22.) New York: Walter de Gruyter. 1990. Pp. x, 254. \$70.00.

Seth Taylor's interesting and carefully reasoned study of how Friedrich Nietzsche's philosophy was interpreted by German Expressionist writers provides further evidence for R. Hinton Thomas's thesis in *Nietzsche in German Politics and Society, 1890–1918* (1983), that Nietzsche's ideas found their most positive reception in Wilhelmine Germany not among the nationalist and authoritarian Right, where they were generally rejected, but among the libertarian and cosmopolitan Left. The successful appropriation of Nietzsche by the nationalist Right in Germany after World War I and his subsequent excoriation by Marxists such as Georg Lukács have obscured the fact that Nietzsche's original influence was stronger on the Left than on the Right. Critics of the quasi-feudal Wilhelmine regime found in Nietzsche's antipolitical precepts the material to combat the militarism, authoritarianism, and illiberalism of German society and the obsequiousness of the German bourgeoisie. Nietzsche's philosophy served as the primary inspiration of Expressionist critiques of an authoritarian value system that served the interests of the state. Immoralism functioned as an emancipatory force in a society where morality served to conceal exploitation and domination. Expressionist writers and thinkers such as Kurt Hiller, Otto Gross, Salomo Friedlaender, Anselm Ruest, and Hugo Ball embraced Nietzschean vitalism, irrationalism, and immoralism not out of opposition to basic liberal values but rather as a means to attain the individual freedoms that the liberal movement in Germany had promised but never delivered.

Taylor's avowed purpose is to combat the notions that Nietzsche's philosophy was the source of the "conservative revolution" and fascist ideology in Germany or that Expressionism, despite its attacks on rationalism, bore any affinity to these right-wing movements. Beyond the problem of Nietzsche's ambiguous legacy lies the larger issue of the genealogy of

fascist ideology. Taylor argues that fascism is less a consequence of an irrational romantic revolt against the classical Enlightenment tradition (as Lukács contended both in *The Destruction of Reason* [1954] and in his polemics against Expressionism) than of the dynamics of that tradition itself and its instrumentalization of reason in the service of power and domination. Through vitalism and the sanctification of instincts, Expressionists (as, in their view, Nietzsche before them) sought to counteract the abuse of reason and the authoritarianism and militarism that it had engendered in Germany. To Expressionists the Nietzschean revaluation of values meant the restoration of individual autonomy that both the deterministic doctrines of positivist science and the stifling conventions of modern society denied. Although Taylor's analysis echoes a major theme of Max Horkheimer and Theodor W. Adorno's *Dialectics of the Enlightenment* (1947, 1972), he does not address the complicated theoretical question to what extent Nietzsche and the Expressionists, by pushing the emancipatory urge of the Enlightenment to such extremes, contributed to the very corruption they were attacking.

World War I served as the catalyst for the destruction of the left-wing Nietzsche cult in Germany and the rise of a right-wing cult. With few exceptions, most notably Hiller, who continued to advocate an activist creed of social reform inspired by Nietzsche, Expressionist writers turned away from Nietzsche as a result of the war. Nietzschean individualism now seemed irrelevant or even antithetical to the urgent task of creating a more democratic society and preventing a recurrence of war. A number of Expressionist writers now embraced Marxism, which before the war they had rejected as the materialistic ideology of the conformist, nonrevolutionary Social Democratic Party. Yet even those who repudiated Nietzsche did so not because they associated him with militarism or reaction, but because his heroic individualism now seemed to be too much a part of the nineteenth-century culture discredited by the war. "Ironically, the creator of the culture critique came to be associated with the very culture he criticized" (p. 223). The defection of left-wing Nietzscheans allowed publicists of the extreme right to exploit Nietzsche's philosophy for their own ends. Taylor does concede, however, that the contradiction between Nietzsche's elite individualism and the political engagement necessary to found a new and more tolerant culture—a contradiction reproduced in Expressionism—inherently limited the possibilities of using Nietzsche's philosophy to achieve left-wing goals.

ROD STACKELBERG
Gonzaga University

ULRICH HERBERT. *A History of Foreign Labor in Germany, 1880–1980: Seasonal Workers/Forced Laborers/Guest Workers*. Translated by WILLIAM TEMPLER. (So-

cial History, Popular Culture, and Politics in Germany.) Ann Arbor: University of Michigan Press. 1990. Pp. xvi, 310. \$37.50.

The recent brutal attacks against foreigners in Germany since the unification have revived unpleasant memories of the controversial issues of forced and immigrant labor in the nation's history. Germany has grappled with these issues for well over a hundred years with depressing results. As Ulrich Herbert points out in this synthesis, the continuity from Imperial Germany to the Federal Republic is striking, especially the socioeconomic exploitation of these foreign workers. Starting with Imperial Germany's attempt to control and use foreign workers, Herbert traces how Germany, first by structural and economic means and later by ideological means, sought to solve its pressing labor problems with imported labor. Dividing German history into conventional periods, the author argues that, except for the Weimar and Nazi eras, Germany has needed an international labor market structured to German advantage—that is, a market that would allow Germany to draw from less-developed, peripheral areas of Europe enough labor for the growing industrial Reich. Sometimes this was done peacefully, as in the pre-World War I period and the period of bilateral agreements (1955–73); sometimes it was done forcefully, as during the world wars; and sometimes it happened accidentally, as with the massive German refugee influx from Eastern Europe after World War II and since the wall came down. In all cases, impersonal economic processes have been markedly influenced by a German ideology that was wary of “foreign infiltration” and, at times, was shrilly “xenophobic.”

Herbert is at his best in the Nazi period where he has already established his reputation with his previous works. In explaining the Nazi foreign labor program he notes significant and substantial differences from the earlier periods. Granting that the Nazi program was developed from the earlier set of practices of Imperial Germany, especially the World War I experiences of using massive numbers of prisoners of war and forced civilian workers from Belgium, Herbert sees the Nazi attempt as but one of several possibilities. What distinguished it was its radical racist aspects, its activism, and its “total abandonment of any traditions of humanism and enlightenment” (p. 190). He also blames the German public for passively accepting it. How successful were these various attempts at utilizing foreign labor? His overall answer is that it was very successful. During both world wars and the post-1960 period foreigners made an important, indeed decisive, contribution that has not been appreciated. Whether the foreigners will continue to be vital to the newly formed Germany is a stickier question. Although Herbert ends his book at 1980, the English translation postscript indicates some disturbing future patterns such as the exploitation of former East Germans much like

refugees after World War II, and a growing xenophobic mood. Herbert's excellent account of the historical development of the use of foreigners over the past century should serve as a timely warning to the newly united Germany, but unfortunately the entire thrust of his scholarship would seem to indicate otherwise.

This is the best brief account available on this complex and nagging subject. Herbert has written an impressive synthesis on a problem that will continue to plague Germany and the future Europe groping toward economic unity.

EDWARD L. HOMZE
University of Nebraska,
Lincoln

RÜDIGER HACHTMANN. *Industriearbeit im “Dritten Reich”: Untersuchungen zu den Lohn- und Arbeitsbedingungen in Deutschland 1933–1945*. (Kritische Studien zur Geschichtswissenschaft, number 82.) Göttingen: Vandenhoeck und Ruprecht. Pp. 464. DM 86.

Over the past fifteen years an increasing number of historians have concluded that German workers not only provided a greater share of Adolf Hitler's vote prior to 1933 than was once thought but also generally cooperated with the Third Reich thereafter. Proletarian opposition, as Wolfgang Zollitsch recently remarked, proved to be “merely a marginal problem” under Nazism (*Arbeiter zwischen Weltwirtschaftskrise und Nationalsozialismus* [1990]). This methodical, valuable, and rather heavy-going book by Rüdiger Hachtmann is an attempt to explain why. According to its author, the regime's encouragement of such “modern” practices as assembly-line production, time-motion studies, and multigrade wage scales both splintered and stimulated the work force, thus turning it to Hitler's aggressive and atavistic purposes. While explicitly enlisting Nazi labor policy under Jeffrey Herf's rubric of “reactionary modernism,” Hachtmann therefore also implicitly extends to workers my contention that manipulated competition was decisive in shaping German business's behavior during the twelve-year reich.

The core of the book is a detailed analysis, based on close examination of extensive archival and statistical sources, of the development of production processes, compensation levels and types, and labor classifications in all German industries except mining, but with special attention to the contrast between the metals and textiles branches. Both within and between sectors, Hachtmann argues, the regime satisfied its competing objectives of encouraging productivity and holding down the national wage bill by offsetting favors to key blocs of workers with stinginess toward others. Thus, armaments makers and German males were steadily advantaged and producers of consumer goods, workers in the service or retail trades, women, and foreign laborers correspondingly deprived.

Moreover, the economy came to function on the basis of a fairly constant number of skilled workers, ever more flexibly defined and strategically located. In consequence, for some German workers, opportunities and benefits rose enough to lend credibility to Nazi propaganda about the people's community. At the same time, the interests of employers and the regime were harmonized, in that regulators were always prepared to turn a blind eye to upward or downward adjustments of official wage scales or job classifications, so long as these served the goals of armaments and autarky.

As befits a well-constructed monograph, this book remains tightly focused on its subject, wage and compensation policy. It nonetheless throws light on a larger issue, the superficial contrast that the Third Reich presents between internal incoherence and external strength. Despite what he finds to have been the polycratic nature of Nazi administration and the absence of a neat, consistent plan governing labor policy, Hachtmann argues that the results accorded remarkably effectively with the regime's ideological objectives. Darwinian conditions were not incompatible with a certain, terrible sort of efficiency.

If this work has a defect, it lies in its exclusive emphasis on materialist explanations for the behavior of German laborers. Such considerations almost certainly shaped the political mood and conduct of workers more strongly than ideology or nationalism, especially when considered in the aggregate over time. They also substantially substituted for open applications of terror. But Hachtmann might have done better to argue these points rather than simply to posit them. And, although he alludes to the phenomenon during the war years, a history of German labor in the Third Reich that faces up to working-class racism remains to be written. Still and all, this richly informative and well-researched work makes an important contribution to our understanding of social policy and its political consequences in Nazi Germany.

PETER HAYES
Northwestern University

GERHARD PAUL. *Aufstand der Bilder: Die NS-Propaganda vor 1933*. Bonn: J. H. W. Dietz. 1990. Pp. 324. DM 72.

Gerhard Paul's work, written in conjunction with Klaus Megerle's multivolume research project on the political culture of the Weimar Republic, is a recent *Habilitationsschrift* of the Political Science Department at the Free University of Berlin. In this major contribution to the study of propaganda, the author is clearly influenced by Ernst Bloch, who contended in his *Kritik der Propaganda* (1937) that National Socialism was a counterrevolutionary movement based on images, not reality. Not only Adolf Hitler but also V. I. Lenin and Joseph Stalin, Franklin Roosevelt and

Ronald Reagan—to name but a few—have understood the importance of merging image and ideology with technological innovation. Paul's eloquently written book is devoted to image analysis of National Socialist propaganda on the road to power and proves that irrational political mythology is often more important to political success than empirically verifiable reality.

The book is organized under five general headings: conception, direction, script, media, and production. This in itself reveals the author's belief that Nazi propaganda, based on the three columns of feeling, faith, and violence, was in fact the aesthetic staging of politics, staging that paralleled and reflected Hitler's predilection for the irrational in both politics and art. Paul's innovative use of illustrations as a counterpoint to his text is most successful. Some eighty-seven photographs, posters, and caricatures transcend the traditional use of historical illustration, vividly evoking the passionate images employed by the Nazis, at once so hateful yet so effective.

National Socialism in the *Kampfzeit* was more a propaganda than an ideological movement, and Hitler was convinced that visual and emotional images had far more popular appeal than the written word. Accordingly, the mass rally became for the Nazis "a theatrical and aesthetically organized community experience" (p. 42). The party appealed to the visual instinct through posters—those of Hans Schweitzer (Mjölner) were on display in the hundreds of thousands—which were seen as a natural extension of the rallies. Heinrich Hoffmann's photographs of Hitler were effective in creating the myth of the Führer as the incarnation of the *Volks* soul. Finally, like the Communists, Hitler took the battle to the streets, believing that violence was a great persuader in the Weimar milieu.

The author provides a detailed history of the organization and personnel of the *Reichspropaganda-leitung*. Organization was vital to the Nazi Party's projection of the image of a rotten Weimar Republic led by liberal traitors and Social Democrat Marxists, manipulated by alleged "international Jewish wire pullers," and assaulted by Moscow's foreign legion, the German Communist Party. Paul, however, makes far too much of weaknesses in the Nazi Party's organization from 1930 to 1933. The Nazi Party did not stumble into power, but was catapulted there in part because of the effectiveness of its organization in image management. Sadly, J. H. W. Dietz Verlag carelessly printed this book with many typographical errors, marring an otherwise commendable embodiment of Paul's diligent research.

JAY W. BAIRD
Miami University
Oxford, Ohio

ROBERT GELLATELY. *The Gestapo and German Society: Enforcing Racial Policy 1933–1945*. New York: Claren-

don Press of Oxford University Press. 1990. Pp. xiv, 297. \$45.00.

Our understanding of the relationship between German society and the Gestapo's functions on the local level increased dramatically with the use of regional Gestapo case files in Düsseldorf by such scholars as Reinhard Mann and Sarah Gordon. Robert Gellately has written the best of a series of recent works on the Gestapo that include able studies by Adolf Diamant, George Browder, and Walter Weyrauch. Like Mann, Gellately emphasizes the importance of public denunciations in the Gestapo's operations. The author relies primarily on Gestapo case files from Würzburg, Lower Franconia, and, to a lesser extent, Düsseldorf. Gellately also consults an astonishing array of other archives. In contrast to Mann and Gordon, he is not interested in demonstrating German opposition to Nazi racial and political policies; rather, he explores the "interaction between the Gestapo, German society, and the enforcing of racial policy" (p. 7).

Over half of the book is devoted to a thorough discussion of Gestapo operations in general and to a review of political and social trends in predominantly rural, Catholic Lower Franconia. The rest of the book examines the enforcement of Nazi racial policies against both Jews and Poles in that Bavarian region. The author's arguments are clear and convincing. The Gestapo was crucial in implementing a system of terror in the Third Reich. Due to its limited personnel, however, which was recruited largely from the traditional political police, racial policies could only be enforced with the help of public denunciations. Almost three-fifths of the Würzburg case files were initiated by denunciations. Most of the public informers came from "the lower end of the social scale" (p. 158), although all social classes were represented. The informants' motives ranged from opportunism to a genuine support of Nazism. With the help of public informers, racial policies were effectively enforced and most social bonds between Jews and non-Jews were dissolved. Gellately also includes a chapter on the enforcement of racial laws against Poles. After 1943, Gestapo case files involving Poles who had violated racial laws decreased dramatically in large part because of the reluctance of the public to inform on Catholic Poles, who were badly needed for agricultural tasks. This illustrates well Gellately's thesis of the crucial importance of public denunciations in the operations of the Gestapo.

The author's thesis is confirmed by Anna Blumberg-Ebel's recent work on special court cases dealing with "political Catholicism," which reveals how diligently the Gestapo pursued cases initiated by public informers. Gellately also modifies some of the findings of both Mann and Gordon. Since Mann excluded Gestapo case files dealing with Jews, his figures for cases initiated by denunciations are much lower than those offered by Gellately. Considering the number of baseless "racial defilement" cases in the Würzburg

files, it is probable that many of Gordon's Gestapo case files dealing with racial violations did not reflect actual opposition to Nazi racial policies. One major limitation with using these Gestapo case files is that one learns little about the reactions of nobles and other elites to official racial policies. Minor irritations include the constant repetition of the thesis in every chapter and the misspelling of *Krieg* three times on three pages (pp. 217–19). In general, however, this is a valuable contribution not only to the literature on the Gestapo but also to the study of public accommodation and cooperation in the Third Reich.

JOHN PETER HORST GRILL
Mississippi State University

SIMONA CERUTTI. *La ville et les métiers: Naissance d'un langage corporatif* (Turin, 17^e–18^e siècle). (Recherches d'histoire et de sciences sociales, number 45.) Paris: École des Hautes Études en Sciences Sociales. 1990. Pp. 260. 220 fr.

Over the long term of the later Middle Ages and early modern period, guilds in Turin sketch a history that is the mirror opposite of that found for other Italian city-states. According to Simona Cerutti, guilds and other such "social bodies" (p. 111) were wholly absent from mercantile and artisan activity in Turin at the time of their formation elsewhere and appeared in Piedmont only in the last decades of the sixteenth century—that is, when the guild had disappeared or was about to disappear in other Italian city-states. Unfortunately, this inverse pattern of guild history is not explained.

Instead Cerutti begins her story with the first appearances of these "corporate" bodies in the sixteenth century and follows their ebb and flow through the middle of the eighteenth century. The vitality of these groups depended largely on the particular political context of Turin: the conflict over power and authority between the Duke of Savoy's court and the merchants and bankers of the municipal city government of Turin. When the balance of power tilted toward the municipality, the merchants commanded "urban unity" (p. 112) little diminished by competing privileges of artisan organizations; when power shifted to the court, corporate life flourished. This early modern history sets out two turning-points. The financial difficulties that followed the plague of 1630 gave merchants and financiers new opportunities to extend their privileges at court and at the same time "control the eventual centrifugal pressures" (pp. 143–44) of artisan organizations. But the coming to power of Duke Victor Amadeus and a severe crisis in the silk industry around 1730 dislodged the corporations from a long period of lethargy; they began to grow in number and expand their authority over the organization of labor. With this change in the relations of power, the mercantile elites lost their control of key institutions in the city gov-

ernment but preserved their identity and "above all" found "a new source of prestige" (p. 230) in charitable giving and participation in the new confraternities of the Counter Reformation.

Unfortunately, Cerutti obscures this fascinating story of social and political structure, the organization of labor, and symbolic forms of action and identity in a thicket of theoretical allusions from "network analysis" to Wittgenstein. What is missing is a systematic analysis of the data. For example, on the basis of three "biographies" Cerutti asserts that after the plague of 1630 the families of artisans became a "closed group" (p. 82) and "the discourse (*langage*) of work was weak" (p. 85). According to Cerutti, by 1730 this *langage* and the sociology of labor had completely changed, but for the later period she fails to supply comparable data. And, from a sample of one hundred merchant testaments selected mostly from the second half of the eighteenth century, Cerutti maintains that after the crisis of the 1730s merchants changed their notion of family to one that shows "the professionalization of this group" (p. 225). Her evidence suggests a supposed change in patterns of property descent from those that favored distant male kin to ones that made wives universal heirs in the absence of surviving sons. Even if the reader is willing to make this interpretative leap, an evidentiary problem nonetheless remains. The earliest testament selected for this group is 1730; Cerutti has failed to collect a comparable data base of merchant testaments from the earlier period of the seventeenth century when merchant notions of family were different and patterns of property devolution presumably "much more diluted" (p. 225).

SAMUEL COHN, JR.
Brandeis University

ROBERTO VIVARELLI. *Storia delle origini del fascismo: L'Italia dalla grande guerra alla marcia su Roma*. In two volumes. (Collezione di testi e di studi, Storiografia.) Bologna: Mulino. 1991. Pp. 651; 952. L. 60,000; L. 70,000.

The first of these two massive volumes on the origins of Italian fascism, which covers the period from the outbreak of World War I until the fall of 1919, was published originally in 1967 as part of a projected three-volume work. With the second volume Roberto Vivarelli takes us another year forward through the occupation of the factories and the rural unrest in the fall of 1920. With the third, uncompleted volume, which he wryly predicts "will be quicker than this one," he intends to lead us through the final two years that culminate in the march on Rome.

Unfortunately, the reason why the second volume took so long, as well as why it is so long (950 pages as against the 650 of the previous one), can hardly make the reader optimistic about the arrival and success of the third. During the quarter century that divides the

two volumes, Vivarelli has had a change of heart about his principle of organization. Apparently, he has come to realize how heavily committed he is against the assumption implicit in the largely chronological approach of the first volume, namely, that the most significant origins of fascism were to be found in the war and postwar years. Since his argument is that its most significant origins lie rather in the half-century between Italian unification and the onset of the war, in the weakness of the liberal state and the gap between that state and the backward civil society it was supposed to lead, he now sees the need for a much longer purview than his original plan provided.

Thus, in the new volume, Vivarelli devotes three-quarters of his 200-page first chapter on the parliamentary elections of November 1919 to giving us the necessary "background" that will support his indictment of the Italian ruling class for failing to prepare civil society for the expanded suffrage being instituted in the political system. Similarly lengthy flashbacks on the weaknesses of Italian socialism and the problems of rural society follow in two of the volume's remaining three chapters. It is a pity that Vivarelli did not recast the entire narrative either as a chronology stretching back to 1861 or as a topically organized review of the problems behind the origins in question. That he has chosen to do neither has left us, unfortunately, with the worst of each world: a chronology blurred by long digressions and a topical approach too subservient to the chronology to emerge with clarity.

A related problem in the enterprise is that Vivarelli has chosen to leave the first volume completely unrevised. This is especially distressing given that about half the overall space is devoted to footnotes, some of which are now quite misleading. Thus, one note directs the reader (vol. 1, p. 260) to the "most complete" guide to the bibliographical literature on Benito Mussolini, an article written in 1963. Similarly, we are referred (vol. 1, p. 510) to a book published in 1956 as the "most recent biography" of Gabriele D'Annunzio, even though attentive readers will have noticed that a biography from 1983 (one that is in fact much better) is cited in the volume's new preface (vol. 2, p. 35).

Vivarelli's main argument about the weaknesses of Italian liberalism, although hardly novel, is clear and convincing, but only when taken as an important part of the story rather than, as he does, the whole of it. Part of the difficulty here is that Vivarelli gives far less attention to weaknesses in the building of civil society than he does to the state. Thus, the efforts of the *Corriere della Sera's* Luigi Albertini and other editors to build a strong liberal press get only a few pages, and prewar cultural movements at national renewal like the Florentine journal *La Voce*, as well as conditions in the universities and schools, are hardly considered at all. Another part of the difficulty is that Vivarelli wants to accord the weak liberal state more blame for fascism than it deserves. He depicts the rise

of fascism as a rather mechanistic Euripidean tragedy in which an inevitable result is produced by the character flaw in liberalism combined with the inherent weakness of new liberal democratic movements, such as the one led in the postwar period by Giovanni Amendola, and the lack of alternative leadership from socialism, ruinously torn as it was between a dominant maximalist faction dreaming of an Italian sequel to the Russian Revolution and a reformist wing too afraid to act on its liberal democratic convictions.

Except for occasional uses of shrewd tactical calculation, Mussolini and the fascist movement are credited with no active role in the events. Mussolini himself gets so few pages in the second volume that readers could easily be excused for not realizing he is part of the story. Even more amazingly, we get next to nothing on the rise of the Fascist Party, a topic whose importance has been clearly demonstrated by the brilliant recent book of Emilio Gentile (*Storia del partito fascista 1919–1922: Movimento e milizia* [1989]). Moreover, Vivarelli's Mussolini is a stick-figure, a "socialist" who nonetheless displayed a "systematic lack of convictions" (vol. 1, p. 262) while leading that party on a disastrously leftward course between 1912 and 1914, and then by 1918, a straightforward "nationalist" and political opportunist. Nowhere is there any reckoning made with the spiritually troubled side of Mussolini and with those painful early experiences in life that would provide him with the rhetorical resources for mobilizing a mass movement. Certainly the leadership of the incumbent political class of Giovanni Giolitti, Antonio Salandra, Vittorio Emanuele Orlando, Francesco Nitti, and their subordinates was inadequate. Yet without appreciating the sources of Mussolini's charismatic side, distasteful to us as it certainly is, we cannot hope to understand why so many otherwise intelligent people not only permitted but also encouraged his rise to power.

Despite these criticisms, Vivarelli's work will be of considerable value to specialists patient enough to hunt through the extraordinary wealth of information he provides. There is a lifetime of learning here, based on both published and archival sources. Vivarelli's use of the latter is far more comprehensive and systematic than in earlier studies with the same focus, particularly in his final chapter on agrarian unrest. Moreover, the new edition is handsomely constructed and contains valuable appendices and a reliable index.

WALTER L. ADAMSON
Emory University

ISTVÁN DEÁK. *Beyond Nationalism: A Social and Political History of the Habsburg Officer Corps, 1848–1918*. New York: Oxford University Press. 1990. Pp. xiii, 273. \$39.95.

This humane, meticulously researched, and attractively written account of the Joint Army officer corps

between the revolutions of 1848 and the onset of World War I will be indispensable to all those interested in the social history of the military, as well as to specialists on the history of the Habsburg monarchy.

In the opening chapters, István Deák outlines the scope of the book and the strengths and limitations of his primary sources. He then offers a concise history of the monarchy and of the role of the military within it down to the ascendancy of Franz Conrad von Hötzendorf. The long decades of peace between Königgrätz and the carnage on the Isonzo River were spent by the soldiers in garrison towns or on pacification campaigns in southern Dalmatia or Bosnia-Herzegovina. The more original parts of the book, based on an analysis of the personal files of some one thousand lieutenants held in the Vienna War Archive, lead the reader through the rites of passage undergone by Deák's officer subjects from the rigors of military training schools, via regimental daily life in godforsaken Galician towns, to death in the trenches.

Palpably enthusiastic for a subject that involved his own father (Ensign Stephan Deák is shown at the Russian front in 1914), Deák's book contains a wealth of information, some of which is presented in the form of diagrams and statistical tables, on army life. Having been trained and inducted, recruits were posted to northern Italy, Vienna if they were lucky, or to Galicia, "a place to get drunk, and to stay drunk" in Deák's memorable characterization (p. 109). Life in the army was also expensive and potentially dangerous. A forgotten courtesy or an imagined slight could and did result in the rituals of pistols or sabers at dawn. These sections of the book include a clear account of the complex workings of courts of honor and a discussion of the ascertainable facts of the Redl affair. Homosexual dealings also figure prominently in a book that is, after all, about an all-male society.

In stark contrast to the more anecdotal passages, Deák concludes with a discussion of the Joint Army in World War I. A staggering 1,016,200 men were killed and a further 1,691,000 captured or missing fighting the Russians in Galicia or the Italians on the Isonzo River. Poignantly, Deák tells of the 300,000 Jews, including 25,000 officers, who served the emperor-king, and of how later they left their medals and memorabilia in their apartments and houses when they went to the Nazi gas chambers. A success in peacetime, but an abject failure in war, the Joint Army "offered a vision of peace, order, tolerance, and continuity" (p. 8). In Deák, these soldiers have found a worthy commentator.

MICHAEL BURLEIGH
London School of Economics and Political Science

HARRIET PASS FREIDENREICH. *Jewish Politics in Vienna, 1918–1938*. (The Modern Jewish Experience.) Bloomington: Indiana University Press. 1991. Pp. vi, 272. \$22.50.

This book by Harriet Pass Freidenreich makes an important contribution to the burgeoning literature on Vienna, but it contains surprisingly little information on Jewish life in the city or on the specific Jewish role in shaping its distinctive postmodernist culture. The study is primarily an "in-house" history of the *Israelitische Kultusgemeinde*, "the official body for legislating and implementing measures to try to improve the Jewish situation" (p. 18). The author contends that the disputatious nature of the Viennese community was more characteristic of Jewish politics outside of Austria than within the First Republic. To prove her point she deftly compares Jewish communal life in the Danubian metropolis with that in the Successor States, Poland, and even contemporary Israel. Given the contentious, fragmented political culture of inter-war Austria, this argument is not altogether convincing. While the acrimonious debates of the *Kultusgemeinde* may have been paradigmatic of Jewish politics, they also mirrored the divisions of Austrian society into rationalist, atavistic, and religious camps.

Freidenreich nevertheless shows that Viennese Jews did develop a voting pattern different from the Austrian norm in that they regularly overlooked their proprietary interests by casting ballots in municipal and federal elections for the Social Democratic Party. Relying on massive archival evidence, she analyzes in detail the geographic, social, and religious fissures dividing the community into Western Jews and *Ostjuden*, believers and non-believers, assimilationists and Zionists, and modern and traditional orthodox. Within the *Kultusgemeinde* itself the major fault line ran between Liberals and Jewish Nationalists, the former regarding Jewish identity in purely religious terms, the latter seeking to define it along ethnic lines. In some of the most interesting pages of her study, Freidenreich explains how a small group of liberal notables managed to retain control of the community for fifteen years after World War I by means of both skillful electoral alliances and a curial franchise that barred women and recent immigrants from voting. Although an increasing number of Jews inclined toward the Jewish Nationalist position, many felt ambivalent about supporting a faction whose program called for diverting meager financial resources to Palestine and whose arguments were, at times, endorsed by anti-Semites. In 1933, when the Zionists at last took control of the communal board, they found themselves confronting insurmountable problems of poverty, dictatorship, and intensified anti-Semitism. Unable to implement promised radical changes, they could neither overcome the internal cleavages of the Jewish community nor protect its members from economic hardship or even physical abuse. After the Anschluss, in a cruel twist of fate, they were charged by the Nazis with the onerous task of helping scatter their flock to the four corners of the earth.

The great strength of this work is its brilliant explication and analysis of the issues confronting the

Viennese Jewish community in the last years of its existence. Although the discussion is unduly abstract in places, devoting far too little attention to the flesh-and-blood drama of individual lives, it certainly accounts for the fragmented, utterly ineffective Jewish response in Austria to Nazi hatred and violence. Since little is known about the *Kultusgemeinde* in the years after the Anschluss, Freidenreich would render a tremendous service by continuing her investigation through World War II. Such a study would represent an important contribution to the general literature of the Holocaust. It would also reveal that, by comparison, Judeophobia is nowhere as lethal or pervasive in contemporary Austria as the author alleges at the end of her present volume.

EVAN B. BUKEY
University of Arkansas,
Fayetteville

WILLIAM O. OLDSON. *A Providential Anti-Semitism: Nationalism and Polity in Nineteenth-Century Romania*. (Memoirs of the American Philosophical Society, number 193.) Philadelphia: American Philosophical Society. 1991. Pp. ix, 177. \$20.00.

William O. Oldson's short work undertakes to explain the antipathy that the Moldavians in particular and Romanians more generally felt toward the Russian and Polish Jews who, having fled the countries of their birth, came to constitute 47 percent of the population of Jassy, capital of Moldavia, by the time Romania achieved independence in 1878. Romanians were jealous of these Jews because they functioned as the middle class of the province, providing services for which Romanian peasants did not have the skills and which the intelligentsia regarded as beneath them. This intelligentsia—with Mihai Eminescu, Romania's greatest poet, in the lead—took the initiative in arguing the anti-Semitic case, of course at their own level of discourse.

What set the argument off was the inclusion of Article 44 in the Treaty of Berlin (1878), proclaiming that the signatories stood for granting citizenship to all residents of Romania regardless of their religious faith. Otherwise, the article ran, the great powers would withhold their ratification of the treaty, ostensibly a minor matter since the Ottoman empire had already accepted the Treaty of San Stefano, imposed on it by the victorious Russian army, and had no choice but to sign at Berlin.

The Romanians at once assumed Article 44 to be a declaration of principle in no wise intended to affect their treatment of the Jewish migrants now settled in Jassy. Otto von Bismarck, urged on by German Jewry, appeared to take Article 44 at face value, but his real purpose was to force Bucharest to buy back the Romanian railway system, built by the Germans but evidently now a money loser. Once the Romanians had bought the lines the German Chancellor

lost interest in the rights of Jassy's 400,000 Jewish immigrants. In consequence only those Jews who were veterans of the war of 1877–78, plus a few who were wealthy enough to pay for it and who were also regarded by ethnic Romanians as “acclimated” to the national culture, were given citizenship (for example, pp. 75–80, 83–89).

Oldson has written an interesting and useful book, objective, well-balanced and based primarily on sources in the Romanian language. Unfortunately, he is sometimes careless with essential facts, as when he repeatedly refers to the Romanians having to trade Bessarabia for the Dobruja in 1878. Actually, only the three southernmost counties, those fronting on the junction of the Pruth and the Danube, were involved. These three counties had been given to Moldavia by the Treaty of Paris (1856) in order to limit Russia's access to Europe's second longest river. And it would have been useful had he pointed out that in Hungary Jewish immigration was encouraged to become an entrepreneurial estate, developing a national commerce and industry as preparation for national independence.

R. V. BURKS
EMERITUS
Wayne State University

NIKŠA STANČIĆ. *Gajeva “Još Horvatska ni propala” iz 1832–33: Ideologija Ljudevita Gaja u pripremnom razdoblju hrvatskog narodnog preporoda* [Gaj's “Croatia Is Not Yet Lost,” 1832–33: The Ideology of Ljudevit Gaj in the Preparatory Period of the Croatian National Revival]. Summary in German. Zagreb: Globus. 1989. Pp. 178.

This book by Nikša Stančić examines the period immediately preceding the Croatian national awakening through an analysis of the various drafts of the movement's best-known patriotic poem and song, *Još Horvatska ni propala* (“Croatia is not yet lost”) and the evolution of the nationalist ideology of its author, Ljudevit Gaj. It is divided into four chapters. The first describes all the manuscript and printed versions of *Još Horvatska* and their provenance, and includes each text and the early musical settings. In the second chapter, Stančić uses a profusion of evidence, including weather charts, to date the first version of *Još Horvatska* and establish when and where it was first put to music. The third and most important part traces Gaj's changing ideas on language and national identity from 1826 to 1835, when the Illyrian Movement began. The last chapter describes the role of *Još Horvatska* during and after the national awakening. This is followed by a long appendix that contains photographs of the various revisions, and pictures of Illyrian leaders and Illyrian dress and artifacts. Stančić has based his book on the Gaj papers and a variety of other primary sources and secondary works. There is little new here, although Stančić does

draw on recent work by Croatian historians, and the literature cited all but ignores the work of foreign scholars. The work is well footnoted, but there is no bibliography or index.

Još Horvatska provides a rough framework for the evolution of Gaj's ideas. In 1830, the historic Croatian lands were contained within various units of the Habsburg empire: Civil Croatia and Civil Slavonia, the Croatian and Slavonian Military Frontier, and Dalmatia. Some of these territories had substantial Serbian minorities. The Croats lacked a modern literary language; instead they had three dialects, each with a separate literary tradition. Since contemporary Slavic linguistics grouped the Croatian dialects either under Slovene or Serbian, Gaj first had to prove that a separate Croatian language existed, then decide which of the dialects should serve as the basis for the modern Croatian language and culture. A language based on the kajkavian dialect of Zagreb, Croatia's political and cultural center, would have had little attraction to those living in the other Croatian territories where the štokavian dialect was widespread. The Croats needed a national identity that reached beyond Zagreb, would not alienate the Serbian minority, might lead to the reunification of the Croatian lands, and could serve as a bulwark against Magyar nationalism. Gaj's Illyrian Movement did just that. It introduced a modern language based on the štokavian dialect, put the national awakening within a Pan-Slav framework, and called the overarching nationality and language Illyrian with subgroups: Croatian, Serbian, and Slovene. Stančić describes this process clearly, and uses *Još Horvatska* to show the widening of Gaj's vision. In the first version, which was written in 1833, Gaj called on young Croatian patriots to awaken a sleeping Croatia. By 1835 *Još Horvatska* had become a call to Illyrian “brothers” in all the historic Croatian lands and neighboring South Slav areas.

What is most important about this book is that an establishment Croatian historian has written a study of a crucial moment in the evolution of Croatian nationalism. Until quite recently this was a very sensitive topic. Although Stančić is still wedded to Marxist jargon when describing social and economic development, the core of this small study is sound.

ELINOR MURRAY DESPALATOVIĆ
Connecticut College

ANDRZEJ BROŻEK. *Niemcy zagraniczni w polityce kolonizacji pruskich prowincji wschodnich, (1886–1918)* [Germans from Abroad in the Colonization Policy of the Eastern Prussian Provinces (1886–1918)]. (Studium niemcoznawcze Instytutu Zachodniego, number 56.) Poznań: Instytut Zachodni. 1989. Pp. 335.

WŁODZIMIERZ JASTRZEBSKI. *Der bromberger Blutsonntag: Legende und Wirklichkeit*. Poznań: Instytut Zachodni. 1990. Pp. 199.

The Polish Western Institute (Instytut Zachodni) in Poznań has sponsored a sizable share of post-1945 scholarship on German-Polish questions. Its substantial publications budget and the anti-German slant of most of its projects have reflected an unpopular, Soviet-imposed regime's belief that to perpetuate the idea of eternal German-Polish enmity was key to its own efforts to justify that regime's existence in Polish eyes. Today, however, with Poland's premier having described German-Polish relations as "better than ever before in history," and Germany serving as liberated Poland's most important economic support, the Western Institute threatens to become an anachronism. Yet while many of the historical works published by this institute have revealed its overriding political purpose, it has also sponsored a good deal of high-quality research at a time when Polish scholars had few other outlets. In brief, the two works under review here—both apparently in preparation at the time of the revolution in 1989 (whose course was not yet known)—represent one example each of these two kinds of Western Institute publication.

Andrzej Brożek's study of the effort to recruit ethnic German farmers from Eastern Europe for the Prussian settlement project in the provinces of Poznań and West Prussia is the outgrowth of two decades of painstaking research; it is a comprehensive investigation of a previously neglected aspect of this topic and a solid contribution to our understanding of the settlement project itself. The first third of the book ranges widely over the establishment of German populations in different parts of Eastern Europe and the reasons for their growing readiness to emigrate in the late nineteenth century. They soon attracted the attention of officials in charge of the settlement project, which was threatened with failure because of the reluctance of most people in their own prosperous and rapidly industrializing country to become farmers in the Prussian east. The number of *auslanddeutsche* settlers remained small until about 1900; after that date, however, they accounted for more than 20 percent of new settlers (and about 35 percent of those recruited from outside the two provinces). In absolute numbers, forty to fifty thousand ethnic Germans were brought "home to the Reich" under this program. Brożek's two most substantial chapters deal with the recruitment effort, 1900–14, and with the war years, when talk of pushing the German-Polish frontier farther east gave added impetus to resettlement ideas. In an effort to place his work in the "continuity" debate, he strains somewhat to suggest parallels between these efforts and the larger-scale relocation of Germans from Eastern Europe under the Nazi New Order after 1939, but his thoroughly researched work remains a significant contribution in its own right.

The fact that Włodzimierz Jastrzębski's much less satisfactory account of "bloody Sunday" in Bydgoszcz/Bromberg (September 3, 1939) was the one chosen for translation into a western language is a reminder

of the political considerations that have traditionally governed the Western Institute. Jastrzębski correctly points out that few historical events have led to such diametrically opposed accounts as the German minority's role during the brief September campaign, in Bydgoszcz as well as in the rest of western Poland. But instead of taking this opportunity to present new evidence or find some common ground between competing positions, he seems content to restate the same basic position that he first expressed twenty-five years ago (and which was spelled out in greater detail more recently by Edward Serwański and others). According to this quasi-official thesis, Nazi Germany instructed the German population of Bydgoszcz and surrounding areas to congregate in the city and mount a "diversionary" attack on Polish forces. On September 3, as Polish forces retreated through the city's streets, they were allegedly fired on by armed German civilians; Polish patriots counterattacked and several hundred Germans were killed, either in the fighting or by execution after being captured "weapon in hand." Jastrzębski recycles the numerous reports of hostile fire—since virtually every street in the Bydgoszcz directory appears in the text, a map of the city would be helpful—but he ignores the dubious reliability of much of this evidence and provides only skimpy documentation. There is disappointingly little on "bloody Sunday" itself; only one thirty-page chapter is devoted to the events of September 3 amid longer accounts of the pre-1939 background and the subsequent trials by the Nazi occupiers of Poles accused of complicity in anticivilian violence.

The problem with this work is that other historians, "third-party" as well as German, present a pretty good case for seeing this event quite differently. They deny that the German minority (which de-germanization measures had reduced from 85 percent in 1918 to only about 7 percent in 1939) was given, or carried out, any military role; they see "bloody Sunday" in Bydgoszcz as a matter of hundreds of innocent, unarmed civilians falling victim to popular rage and panic after police abandoned the city on September 3. (Jastrzębski comes close to this view at one point when he notes that "the active participation of Poles in the struggle with the diversionists . . . sometimes crossed permissible borders, a result of the release of . . . existing tensions and . . . disappointment with defeats at the front" [pp. 158 and following].) But if any Polish units came under fire in the confusion of retreat, it was from the *Wehrmacht* or other Polish units; the local German population had neither arms nor orders to participate in the fighting, and not even under the subsequent occupation regime did any German inhabitant of this city report (or boast of) a role in the fighting.

The eruption of popular anti-German violence in Poland in the first days of the war was subsequently exploited and exaggerated by Nazi propaganda. The officially proclaimed death toll of 58,000 was effectively refuted by Karol Pospieszalski thirty years ago;

he concluded that "only" about 2,000 German civilians had died of nonmilitary causes. A decade later, Peter Aurich wrote the definitive German account, suggesting a figure of 4,000 to 5,000 (including 366 in Bydgoszcz), which would seem to be the logical point of departure for Jastrzębski's latest contribution. But while he provides a skeptical summary of Aurich's findings in a concluding chapter (which provides a useful summary of the controversy), he does little to address or refute them directly; he has not consulted Western archives and his bibliography contains no third-party accounts (for instance, Louis de Jong's standard study). Thus, while this summary of the formerly quasi-official Polish position will be helpful to those who do not read Polish, those who would want to press ahead with the effort to reconcile the two opposing positions will find Pospieszalski and Aurich more helpful.

RICHARD BLANKE
University of Maine

S. A. CHIBIRIAEV. *Velikii russkii reformator: Zhizn', deiatel'nost', politicheskie vzgliady M. M. Speranskogo* [The Great Russian Reformer: The Life, Work, and Political Views of M. M. Speranskii]. (Istoricheskie portrety.) Moscow: Nauka. 1989. Pp. 213. 1 r.

Russian historiography—whether prerevolutionary or Soviet—is notoriously deficient in biographies of the country's most important public figures. It is, therefore, a source of gratification to see our Russian colleagues turn at last to historical biographies. M. M. Speranskii (1772–1839) was surely one of the most influential officials of the first half of the nineteenth century who helped shape the bureaucratic machine of the empire, to collect and digest the corpus of its laws, and to suggest far-reaching administrative, economic, and political reforms (that, unfortunately were not always implemented in his spirit). He is also one of the few imperial administrators to have had several biographic studies published before 1917 and in the West. For the former Soviet Union, however, S. A. Chibiriaev's essay is a first, although several aspects of Speranskii's administrative activities have been treated within the broader framework of Alexander I's reign (see, for example, works by B. S. Osherovich, A. V. Predtechenskii, S. V. Mironenko), and some of his reform projects published (S. N. Valk, ed. *M. M. Speranskii—Proekty i zapiski* [1961]).

Chibiriaev is concerned exclusively with Speranskii's public career, as is only right, for his private life was almost entirely subsumed in his work. It is a pity, however, that the author believes it undesirable (or impossible) to investigate Speranskii's intellectual and spiritual life, a life that had some interesting philosophical and religious components. It is also a reason for Chibiriaev's inability to deal comprehensively and critically with Speranskii's political ideas, despite the

avowed purpose of the subtitle, whether within a purely Russian or an all-European perspective.

But let us be thankful for small mercies and note that Chibiriaev is factually quite accurate and does cover, albeit superficially, most of Speranskii's multifarious administrative activities. He is best on the organization of the Council of State and on the reorganization of the ministries, and least satisfactory on codification. For the latter he seems to lack the most elementary knowledge and competences and relies entirely on M. M. Vinaver's short monograph (1908) that deals with the sources and characteristics of Book X of the Digest of Laws. In any event, Chibiriaev's account of Speranskii's work relies entirely on secondary materials and some published sources—and incompletely at that, for there is no reference even to the Soviet studies mentioned above. He ignores the wider historical and chronological framework, as well as the Western literature on the subject and the period (one of my own books appears in the bibliography but does not seem to have been used). The slender book of 150 pages of text is interspersed with illustrations and complemented with a photographic reproduction of Speranskii's so-called Perm' letter to Alexander I and fragments from his *Treatise on Rhetoric*.

Hopefully the book will prove to be an early example of a growing biographical literature on Russia's seminal personalities. It may, therefore, not be amiss to point to its negative features; not as carping criticisms but to direct attention to those failings that are in greatest need of correction. First, there is a lack of chronological clarity (to cover up unpleasant facts), not only in the organization of the material but also in the examination and discussion of specific issues. More serious is the constant recourse to ideologically charged—and inaccurate— cliché words (feudal, reactionary, aristocratic, liberal, intrigue, capitalist, conspiracy, and the like). Annoying too is the use of encomious epithets (outstanding, progressive, patriotic) instead of a critical examination of the evidence. A loose vocabulary and disregard (or ignorance) of the intellectual and European context make for questionable or overly sweeping generalizations (nature of the French Revolution and of its impact, the Decembrist movement, military colonies, Arakcheev's role). It is in fact a form of narrow and chauvinistic ethnocentrism that results in isolationist perspectives and reflects an unwillingness to assess Russia's place in the history of Europe in the first half of the nineteenth century.

But let us end on a more positive note. To study and praise Speranskii is also to make a contemporary statement: reforms should start with the central political and administrative system to pave the way for a gradual, but thorough, transformation (perestroika) of socioeconomic reality.

MARC RAEFF
Tenafly, New Jersey

ROBERT SERVICE. *Lenin: A Political Life*. Volume 2. *Worlds in Collision*. Bloomington: Indiana University Press. 1991. Pp. xviii, 422. \$39.95.

PHILIP POMPER. *Lenin, Trotsky, and Stalin: The Intelligentsia and Power*. New York: Columbia University Press. 1990. Pp. xiii, 446. \$16.50.

As Soviet communism dies, the debate over the character of its founders has revived, enhancing the significance of these two works. The second volume of Robert Service's masterful biography of V. I. Lenin is the more substantial contribution to that debate. Based on intensive study of the available primary sources, including those published since 1985, his work covers Lenin's life from 1910 to the signing of the Treaty of Brest-Litovsk in March 1918.

The greatest strength of Service's study lies in the subtlety of its analysis of Lenin's personality. Service has managed to maintain his emotional distance, judiciously assessing Lenin's considerable strengths and weaknesses without becoming obsessed by one or the other. The subtitle, "A Political Life," conveys the essence of Service's interpretation. He sees Lenin as a man whose concerns were ultimately political, whether he was fighting with other émigrés, writing philosophy, or making a Draconian peace with Germany. This is not the same as asserting that Lenin was power hungry, although he was, or that he was autocratic by nature, which Service also acknowledges. But Service argues that it is equally important in understanding Lenin to keep in view his deep commitment to abolishing the social injustices of prerevolutionary Russia and his devout Marxism.

Service lays considerable stress on the nature of Lenin's Marxism. Lenin's dedication to Marx's thought was religious, priest-like, possessive; he once described himself in a letter as "in love with Marx" (p. 98); hence his tendency to defend his own reading of Marx with a fervor of which he himself was practically unaware. Although capable of manipulating others' weaknesses, Lenin was not psychologically insightful, least of all about himself, so the emotional depths from which his belief system welled remained hidden to him.

This is not to say, Service argues, that Lenin's commitments were not rational or ethical; they were both. But they also expressed the deeper layers of his personality: his need for ideological certainty, his astonishing self-confidence, as well as his persistent "naïve expectation, and failure to think issues through" (p. 143) that Service demonstrates both in Lenin's prerevolutionary writings and postrevolutionary politics. Lenin's naiveté, which made possible his faith in the inevitability of the socialist utopia, also protected him from realizing the appalling implications of his more cold-blooded proclamations and policies.

Service develops this interpretation by following Lenin across the years 1910 to 1918. Discussing the prerevolutionary fighting between Mensheviks and

Bolsheviks, Service portrays Lenin as incorrigibly schismatic, but he also demonstrates the ineptitude of Mensheviks in dealing with him. Once the revolution began, the doctrinaire Lenin developed a new political finesse, adjusting quickly to exigencies and possibilities that frightened those who were both less dogmatic and less flexible. On seizing power he became with astonishing swiftness a competent administrator and adroit mediator of disputes. Just as quickly, he sought unlimited police powers for his fledgling government. Thus, the fractious intellectual transformed himself into one of the major political figures of the twentieth century.

Throughout Service carefully assesses the world against which Lenin reacted, both the immediate one within the party and the larger Russian and European context. This permits him to trace the development of Lenin's thought through war and revolution, and he skillfully explicates Lenin's theoretical and polemical works both as expressions of his personality and ideology and as responses to immediate circumstances.

If the study has any weakness, it is that it is so centered on Lenin that it slights the remarkable personalities among whom he worked. Service discusses other Social Democrats only when it is necessary in explaining Lenin's behavior, and then briefly. Leon Trotsky's conversion to Bolshevism, for instance, is barely mentioned. Although Service demonstrates throughout that Lenin was far from dominating his party, he leaves the other Bolsheviks largely in the shadow of their leader. His portrait of that leader, however, is compelling.

Philip Pomper's work examines the psychologies of Lenin, Trotsky, and Joseph Stalin from their childhoods until Lenin's death in 1924 in an effort to understand "the psychology of power-seeking" (p. 280). Pomper portrays Lenin as driven by a will to power and control, Stalin by a will to power, but also by a heroic image of himself as the clever underdog, and Trotsky by a taste for glory and, failing that, martyrdom. He derives this analysis from a reconsideration of the subjects' published works, Trotsky's archives, and major memoirs.

Pomper's interpretation of Lenin is not original. He is one of those who sees Lenin's autocratic personality as the source of Bolshevik dictatorship. "Ultimately, his style of leadership created the groundwork for disaster, and he showed no capacity for changing it, even on his deathbed" (p. 356). Pomper is stronger when dealing with Trotsky. He provides an interesting discussion of the psychological dimensions of Trotsky's transformation into Lenin's right-hand man, and he examines far more than others the extent to which Trotsky's Jewishness affected both his own psyche and other Bolsheviks' perceptions of him.

Pomper's interpretation of Stalin is his most original. Rejecting Robert Tucker's premise that Stalin identified with Lenin, Pomper portrays Stalin instead as identifying throughout his early career with heroic

Georgian figures such as the fictional Koba, whose name he took as an underground alias. When Stalin became a Marxist, he began to model himself on Social-Democratic leaders who had risen from humble origins, such as August Bebel. Pomper argues that Stalin's relations with Lenin were complicated by Stalin's own need for independence and by the class and cultural differences between them so that, far from being a loyal disciple, Stalin often chafed under Lenin's schoolmasterly tutelage.

Unfortunately the documentation for these interpretations is unavoidably skimpy. Trotsky did not write about his Jewishness or Stalin about his irritations with Lenin. At a more fundamental level, all three men obviously had a will to power; they demonstrated that in 1917. The origins of that will Pomper traces to their childhood experiences with parents and siblings, but however carefully he probes their early lives, he can only speculate on the psychological dynamics of their families. The evidence, particularly in the case of Trotsky and Stalin, simply does not permit anything beyond speculation.

In sifting among the records for their darker natures, Pomper overlooks his subjects' rational and ethical motives and the ideology that shaped their perceptions. He also consistently undervalues the impact on them of the world in which they lived. In Pomper's view some revolutionaries may have been driven by a moral revulsion at tsarist atrocities; others may have sought to build a better world. These three men sought power. Lenin's analyses were never cogent; they were always cunning. Trotsky became a ruthless commander of the Red Army to compensate for his Jewish doubts about his own masculinity. As such oversimplifications accumulate, the effort to explain the origins of political ambition circles around to the old charge that the Bolsheviks were despots from the cradle.

Service provides a better explanation of Lenin's evolution into a dictator, one that acknowledges the man's weaknesses of character but also considers the shaping effects on him of his beliefs and his world. Only such balanced assessments will permit us to understand the great Russian tragedy that transformed revolutionaries into autocrats and utopian dreams into calamities.

BARBARA EVANS CLEMENTS
University of Akron

WILLIAM CRAFT BRUMFIELD. *The Origins of Modernism in Russian Architecture*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xxv, 343; 40 plates. \$75.00.

Despite an endless fascination for the last days of the Old Regime in Russia—the politics, naturally, but also its society and ideas, literature, music, and painting—nothing of consequence has been written in the West about architecture. William Craft Brumfield's book is

intended—and succeeds admirably—in filling this void. Only Elena A. Borisova and Grigory Sternin's *Russian Art Nouveau* (1988) touches on similar themes and equals it in pictorial beauty.

Brumfield does not, of course, limit himself to cover the entire field of Art Nouveau, or style moderne, as he chooses to call it. Rather, his is a description and interpretation of Russian building styles between the 1880s and the revolution. During this period, when Russia was entering or attempting to enter the industrial age, those enduring cultural tensions (Russia, whither East or West?) were as intellectually destabilizing as ever. Besides problems of incorporating new technologies and building materials in their work, architects sought a "unifying idea," a national style befitting Russia's coming of age.

In successive chapters Brumfield covers "The Quest for a National Style," a contest between protagonists of historicism and eclecticism; "Russian Architectural Criticism and the Propagation of the 'New Style,'" a survey of architectural critics and new architectural journals; "The Style Moderne in Moscow," an appraisal of the building actually undertaken; "Fedor Shekhtel," the style moderne described and judged in terms of its most creative practitioner; "The Ambiguities of the Moderne in St. Petersburg," departures from the Moscow mode in Petersburg where the Finnish School and neoclassicism competed; and "Modernism Reconsidered," the Palladian triumph in the capital.

This is an important work in cultural, intellectual, and architectural history. The author nicely balances description and interpretation, relates the text especially well to the illustrations, and establishes a historical context. He notes, for example, the criticism that Russian architects of these years were largely dependent on private funding and consequently focused on building and decorating exquisite homes and apartment houses rather than improving the urban infrastructure. Brumfield also balances admiration and sympathy for moderne architects, who sought but failed to settle on the "unifying ideas" in their own day, with an appreciation of their influence in our time.

That this work is graced with excellent and pertinent pictures is a tribute to Brumfield's considerable reputation for architectural photography. Certainly, the reader will be pleased with forty color plates and 323 black-and-white photos taken by Brumfield himself or obtained, when necessary, from the archives. Two maps—city plans of Moscow and St. Petersburg—and an excellent index are also crucial aids to the reader.

ALBERT J. SCHMIDT
EMERITUS
University of Bridgeport

JOSEPH BRADLEY. *Guns for the Tsar: American Technology and the Small Arms Industry in Nineteenth-Century*

Russia. DeKalb: Northern Illinois University Press. 1990. Pp. xi, 274. \$27.50.

Joseph Bradley's book is a case study of technological change and modernization within the nineteenth-century Russian small arms industry. He explains why Russia needed to modernize its small arms industry and why native technology failed to keep pace with foreign innovations. He examines the state's military needs, the level of training and tactics in the army, the weapons systems available, and their means of manufacture. He describes how these variables affected the development and procurement of new infantry weapons systems based on the breechloading principle. In a word, his emphasis is on context: technological, military, social, and economic. And it is this emphasis that impels Bradley to plumb the deeper currents of change that swirled around the transition from muzzle loaders to breechloaders within the Imperial Russian Army.

The result is a fascinating perspective that takes the reader from America to Russia, from the advent of interchangeable manufacture at the Harper's Ferry and Springfield arsenals to the persistent legacy of craft manufacture at their Russian counterparts in Tula and Izhevsk. Bradley details the evolution of the Russian craft system, then explains why it failed to meet the test of adaptation to modern small arms production. At the same time, he does not overlook the shortcomings and idiosyncrasies of Eli Whitney and Samuel Colt. His account reveals a fundamental understanding of the complexities of novel weapons systems and their manufacture to include the knotty problems associated with the introduction of new methods and techniques, notably standardization, interchangeability of parts, and the intricacies of producing metallic cartridges. He builds on the landmark works of scholars such as Merritt Roe Smith and translates their concerns into the Russian context. Bradley also highlights the military quandaries associated with technical application and tactical innovation. For the uninitiated, his book serves as a primer on several levels, ranging from tactical to technical and social.

Among concerns of enduring interest are issues of adaptation, procurement, and technology transfer, including sales and marketing. In brief, Bradley explains why Colt pioneered but Berdan and Smith and Wesson won the contracts. As foreign systems gained acceptance and prompted fundamental change, their domestic manufacture added up to a revolution in small arms production and its supporting socioeconomic infrastructure.

Unfortunately for the Russians, solutions to armaments problems lacked persistence and could not ensure successful application. Tactically, the Russians adapted more slowly than their small arms industry. Technically, the revolution that produced the "Berdanka" had scarcely put its products into the field

before the advent of smokeless powder and magazine-feed systems elicited yet another round of change and adaptation. That the Russians could even begin to cope successfully with a second round was remarkable in itself in view of their society's limited adaptive capacities. New challenges also revealed the shortcomings inherent in selective and limited change. If army and armaments industries only belatedly and imperfectly had joined the marathon to modernization, how far could they run while tethered to a backward host society? Or, when would additional requirements for change stretch to breaking fragile linkages between what the Soviets would later call "the peasant rear" and the fighting front?

Bradley has made an original and enduring contribution to the historical literature on technological change and adaptation. His book is "must" reading for students of Russian history, military history, history of technology, technology transfer, and economic modernization. It is soundly conceived, solidly researched, and well written. He deserves credit for writing a first-rate account of an important facet of Russian history with all its complexities, achievements, frustrations, and ironies.

BRUCE W. MENNING

*U.S. Army Command and General Staff College,
Fort Leavenworth, Kansas*

MICHAEL MELANCON. *The Socialist Revolutionaries and the Russian Anti-War Movement, 1914-1917*. Columbus: Ohio State University Press. 1990. Pp. xii, 368. \$39.50.

Over the past two or three decades, our image of the Socialist Revolutionaries (SRs) has been changing. Traditionally, the SRs, in contrast to the worker-oriented Social Democrats, have been portrayed as an agrarian socialist party, concerned primarily with the Russian peasantry, with terrorism among its most cherished tactics. Recent scholarship, however, has altered this picture. Thanks to the work of Oliver Radkey (the foremost historian of the SR party), Manfred Hildermeier, Maureen Perrie, Christopher Rice, and Michael Melancon (the author of the present volume), we have come to see that the SRs directed their efforts less to the peasantry than to the urban factory workers, among whom they gained a considerable following. Moreover, for all their terrorist exploits, conducted largely during the early years of the century, they set their main hopes on the unsensational methods of organization and propaganda to achieve their revolutionary objectives.

In this book, Melancon continues the process of historical revision in which he has been an active participant. Focusing on the war years, he examines the activities of the SR party, in European exile as well as in Russia itself, from the outbreak of hostilities in 1914 through the February Revolution of 1917. Dur-

ing this period, Melancon shows, the SRs, although the only party to engage in revolutionary propaganda among the peasants, devoted their greatest energies to winning over the urban working class. SR organizations operated mostly in Russia's cities and towns, rather than in the countryside, and acquired a dominant position in a number of important trade unions, rivaling the Social Democrats (Bolsheviks and Mensheviks) in overall influence among the workers and surpassing them within the armed forces, both in the rear and at the front.

In the conduct of antiwar propaganda, Melancon maintains, it is again the SRs, not the Social Democrats, who must be accorded pride of place. Both inside the tsarist empire and in the European emigration, the left wing of the SR Party waged a relentless campaign against the Russian war effort, organizing conferences, carrying on agitation, and issuing a stream of leaflets and proclamations denouncing the war as a capitalist struggle for power and profit, with the toilers serving as cannon fodder. Only one type of warfare was acceptable, the SR militants declared, and this was the "social war" that would overthrow the tsarist order. By disseminating propaganda of this sort, argues Melancon, the SRs "helped establish in the minds of workers, soldiers, peasants, and students an identity of anti-war and antigovernment feelings." In so doing, Melancon adds, they helped set the stage for the February Revolution, in which, given their strong influence among the workers and soldiers, they "arguably had the widest role" of all the socialist parties and groups (pp. 2, 277).

By emphasizing the role of the SRs in the antiwar movement and in the February Revolution, Melancon provides a useful corrective to the claims of Soviet historians that the Bolsheviks exercised a predominant influence in both. Yet, in his eagerness to give the SRs their due, Melancon presses his case too far. Not only does he exaggerate the influence of the left SRs, who opposed the war, while minimizing that of the right SRs, who supported it, he also exaggerates the role of the SR Party as a whole in bringing about the collapse of the autocracy. In this regard, he pays insufficient attention to the underlying political, social, and economic causes of the February upheaval, such as the corruption and mismanagement that hindered the conduct of the war, the growing shortages of food and fuel, and the rising toll of dead and injured at the front. For all Melancon's assertions to the contrary, the February Revolution was essentially a spontaneous affair, neither organized nor directed by any radical group or combination of groups, which were pulled along by the tide of events and remained in disarray throughout the crisis. This is not to say that the SRs—or the Social Democrats, for that matter—had no role in the February revolt. But decimated by arrests and plagued by internal divisions, they were unable to assume leadership of the revolu-

tionary movement or to guide it toward any preconceived goals.

PAUL AVRICH
Queens College,
City University of New York

WILLIAM B. HUSBAND. *Revolution in the Factory: The Birth of the Soviet Textile Industry, 1917–1920*. New York: Oxford University Press. 1990. Pp. viii, 227. \$32.50.

This book explores the textile industry as a paradigm of the issues and problems that bedeviled the Bolsheviks' attempts to restructure and revitalize Soviet industry, issues and problems whose genesis was in the prerevolutionary legacy and persisted at least through the first Five-Year Plan. It is a tale of the precarious balance between delivering the promises of the revolution and creating a functioning economy and society that conformed to ideology as interpreted by the Bolsheviks. The dynamics of that balance in the textile industry were those of all industry writ large.

William B. Husband weaves into his narrative the political and economic characteristics of the period under consideration, the turbulence and chaos of the revolution and the Civil War that at every moment created competing imperatives to the problems of industrial reorganization. He reminds us that the general economic deterioration of the period created horrendous problems of supply that both underlay and transcended all attempts to create a new economy and a new society. With what were workers supposed to produce and with what could they be rewarded?

The organs created to restructure the textile industry reflected and exacerbated the chaos of the period. Layer on layer of state, party, and union organs, often overlapping in function, competed for turf, not from crass motives of abstract power but from differing assessments of the goals of the revolution and the means to achieve them. The further the organ from the shop floor, the less likely it was to respond to the workers' immediate needs and aspirations: simple survival and antipathy toward old enemies in new clothing, that is, the technological and administrative experts. Even elected representatives of factory committees, Husband maintains, soon found themselves at odds with the mass workers' aspirations.

The textile industry differed from other industries in significant ways. Husband discusses these differences—of history, scale, organization, geography, and work-force composition—in an admirably succinct, lucid, and intelligent way. The work force drew, more than many other industries, on the rural population, which was less literate, less politically conscious (as defined by the Bolsheviks), and less cohesive both geographically and in spirit. The lack of qualified personnel to carry out the tasks of organi-

zation was at all levels an underlying and undermining problem.

Husband might have heeded a maxim that applies to historical writing as much as it does to fiction: do not tell, show. By concentrating on institutional conflicts, mainly through edicts, communiques, and conference polemics of state, party, and union organs and between the levels of these organs, we are mainly told, not shown, what workers wanted. An example of the many tantalizing suggestions in the narrative is a short allusion to workers' courts whose job it was to address workers' breach of discipline as well as to defend them against supervisory abuse. Clearly, workers did not acquiesce robot-like to party, state, and union wishes. But illustrations and analysis of the language and the behavior of the resistance are meager.

I wish the author had not restricted himself to 166 pages of text. Perhaps he will return to his enormously rich source material to expand and elaborate the interesting and important themes on which this book is based.

ROSE L. GLICKMAN
State University of New York,
Buffalo

I. P. LEIBEROV and S. D. RUDACHENKO. *Revoliutsiia i khleb* [Revolution and Bread]. Moscow: Mysl'. 1990. Pp. 222. 1 r. 90 k.

This book is a study of the influence of the food-supply crisis on revolutionary working-class politics in Petrograd during 1917. On internal evidence, I would guess that the first two chapters, on the February Revolution itself, were written by I. P. Leiberov, while the second half of the book, which takes us through the rest of the year, was prepared by S. D. Rudachenko.

In the portion of the book on the February Revolution, the food-supply crisis is used to stress the coming together of the "class-conscious proletariat" with the "general democratic masses"—in other words, the workers under the influence of socialist parties with the less-organized workers, the lower middle class, the students, and the soldiers. The manifest incompetence of the government linked the frustration of the wider population to the revolutionary aims of the more militant workers. Leiberov provides the best description I have read of the details of the rapidly deteriorating food-supply situation in Petrograd during January and February 1917. Otherwise, there seems little that is new in this part of the book.

The chapters by Rudachenko give us a wealth of details on the changing food-supply situation, the institutional framework of official food-supply organizations, and the wide-ranging activities of the "democratic organizations" (local soviets, factory committees, and cooperatives). The description of working-

class attitudes strengthens the analysis in my own study of the food-supply crisis (*Bread and Authority in Russia, 1914-1918* [1990]). As in February, the food-supply crisis acted as a radicalizing force, not just in the sense that it made people angry but also because it made class war remedies more plausible. The Kornilov rebellion of late August seemed to confirm the evil intentions of the bourgeoisie: not only did it coincide with a drastic deterioration of food supply throughout the country but (as Rudachenko shows) it also led directly to a shortage of supplies in Petrograd. If bourgeois sabotage was the problem, then extensive worker control and, finally, a proletarian state authority was the answer.

The major deficiency of this part of the book is the interpretational framework, which never gets beyond the Bolshevik propaganda themes of 1917: food-supply difficulties are primarily due to the cunning of the bourgeoisie or the cowardly incompetence of the moderate socialists. The analysis relies heavily on the notorious weasel-word "sabotage." The democratic organizations are invariably resourceful and disciplined. All this is rather irritating to the reader who knows that the Bolshevik recipe of revolutionary vigor did not remove any of the genuine dilemmas confronting the Provisional Government and added a few new ones.

Nevertheless, the Western reader, whose attention is more often focused on later Bolshevik failures, might learn something from this book. The severity of the food-supply crisis in autumn 1917 was in itself enough to reveal the Provisional Government as a non-option. Furthermore, Bolshevik insistence on elimination of free trade in basic foods and on state control of distribution—usually ascribed to fanatical "war communism"—grew directly out of working-class attitudes and demands already visible in 1917.

This is a competent monograph of the pre-glasnost variety. Beyond the themes mentioned, it will be of interest mainly to students of revolutionary Petrograd.

LARS T. LIH
Wellesley College

WILLIAM MOSKOFF. *The Bread of Affliction: The Food Supply in the USSR during World War II*. (Soviet and East European Studies, number 76.) New York: Cambridge University Press. 1990. Pp. xvi, 256. \$49.50.

This is, I believe, the first book in English to be devoted to food distribution in the Soviet Union during World War II. It is informative, readable, and objective. In the absence of any other scholarly study of comparable range, it must be adjudged valuable.

William Moskoff's "fundamental argument" is that the central government decided at the outset not to administer the distribution of food to the civilian population. Procurement was organized nationwide for the army and navy, but civilian distribution was

left to local authorities (pp. 2–3, 94–98). Moskovoff does not say what he means by local—whether the work of procuring and distributing was carried on at the city, raion, oblast, or republic levels.

In any case, the central government began in 1943 to interest itself in civilian diets. The reason, says Moskovoff, was that vast areas of the country were recaptured from the Germans and large-scale measures of relief and restoration had to be undertaken (p. 207). We are not told what these measures were; rather, Moskovoff presents only some anecdotes and (unreliable) statistics that reflect life in the war-torn areas. In fact, Moskovoff's book gives very little attention to administrative actions of any kind, perhaps because his narrations are confined almost entirely to 1941–42 when, presumably, the central government was doing relatively little to administer the food supply.

Still, Moskovoff's book is well worth reading. The fashion among Western scholars has been to ignore Soviet achievements, and after more than forty years we have not even begun to understand how Stalin's regime got through the war against Germany. Writers who deal with the subject invariably cover it up with a few remarks about how tough the Soviet people are. Under this badly worn cliché, methods, policies, networks, and structures are tacitly ignored (or their very existence denied). I for one have been hungry for any information that will help explain how a million people were evacuated from Leningrad in only a few months' time (pp. 185–206); how huge numbers of farmers, farm machines, and farm animals were brought out of the western areas in 1941 as the Germans swept in (pp. 21–35); and how refugees were resettled and machinery put into operation in the east (pp. 35–40). Moskovoff does not explain how these things were done any better than anyone else has, but he does rather well at depicting the achievement, and he conveys the immensity of the problems involved. One can hope that some young scholar may be moved by Moskovoff's account to seek out explanations for it.

GEORGE YANEY
University of Maryland,
College Park

GALIA GOLAN. *Soviet Policies in the Middle East from World War Two to Gorbachev*. (Cambridge Soviet Union Paperbacks, number 2.) New York: Cambridge University Press. 1990. Pp. viii, 319. Cloth \$44.50, paper \$14.95.

Galia Golan attempts a herculean task: to discuss decades of Soviet relations with the central Arab lands, Turkey, and Iran and also the Kremlin's attitudes toward the Arab-Israeli dispute, Islam, Communist parties in Middle Eastern countries, and the impact of the Gorbachev years. Furthermore, Golan

seeks to treat all this in fewer than 300 pages. The results are uneven.

The most successful sections deal with Egypt, the Arab-Israeli dispute, and the nature of Soviet interests in the Middle East. These topics account for almost half of the book. In these sections the author discusses major developments in clear prose, showing admirable judiciousness in her explanations of Soviet behavior. The author rejects clichés about Soviet policy that claim Moscow was omniscient, maximally aggressive on all occasions, and infinitely capable of manipulating all developments in the Middle East to its own advantage. To cite but one of many examples, she deftly debunks the argument, widely heard a decade ago, that the quest for sources of oil was a prime motive of Soviet Middle East policy (pp. 15–17). She repeatedly summarizes and evaluates interpretations by well-known observers of Soviet matters. When the evidence remains inconclusive, she is willing to acknowledge the ambiguities.

The rest of the book is not up to the same level. Part of the problem lies in condensing so many other subjects in such a limited space. Too often their treatment seems perfunctory. More than that, Golan does not show the same feel for the nuances of these other subjects. There are even errors of fact (as in the assertions about Soviet claims to Iran in the Molotov-Ribbentrop pact or long-standing drives to acquire a warm-water port via Iran [pp. 29, 176]). These errors exist elsewhere in print; the problem lies in the transmission of misinformation from author to author, none of whom has done original research on the topic.

A fundamental question about this book is its intended audience. If it is meant as a textbook or for others seeking an introduction to the field, then its strongest chapters may prove quite useful. It has, however, little to offer the specialist, who is unlikely to learn something new on major issues. The usefulness of details on specific points is hard to assess because there are no notes to identify sources of information, only chapter bibliographies listing a few works and a cumulative bibliography. Thus, when the author discusses speculation that Moscow supported Baghdad's anti-Kurdish measures early in the 1970s (p. 159), the reader lacks the information to assess the credibility of this anonymous speculation.

According to the bibliography, this book is not a product of original research but a synthesis of the secondary literature and other works published in English. Of the few Soviet sources in the bibliography, all are listed as translated into English; if the author has consulted some of the many sources that have not been translated, the book does not make that known. Although the bibliography lists many major works in the field, there are also notable omissions. For example, there is a chapter on communism in the Arab lands but the author does not appear to have consulted either Joel Beinin and Zachary Lockman's *Workers on the Nile: Nationalism, Communism, Islam and*

the Egyptian Working Class, 1882–1954 (1987) or Selma Botman's *Rise of Egyptian Communism, 1939–1970* (1988).

Thus, the nonspecialist reader, who can benefit most from the book's strengths, by definition will not have the background to know when to be wary. Those who have the background will also have read many of the works the author synthesizes in this book.

MURIEL ATKIN

George Washington University

NEAR EAST

ALBERT HOURANI. *A History of the Arab Peoples*. Cambridge: Harvard University Press. 1991. Pp. xx, 551. \$24.95.

"The Arabic-speaking parts of the Islamic world," Albert Hourani believes, have "sufficient unity of historical experience" to justify treating them as a unit of study (p. xvii). The nature of this unity is indicated in the prologue summarizing the career of Ibn Khaldūn. The area stretching from Spain to southern Arabia to Damascus was a world in which the rise and fall of dynasties was commonplace and the cataclysmic conquests of Tamerlane possible; it was also a world in which "a family from southern Arabia could move to Spain, and after six centuries return nearer to its place of origin and still find itself in familiar surroundings" (p. 4).

Hourani divides the historical experience of the Arab peoples into five ages. The first comprised the period from the seventh to the tenth century, when the roots of the empire, created by armies from Arabia, were established from Spain to central Asia. These armies brought with them Islam and the Arabic language as the vehicle of religion and high culture. The second age extended from the eleventh century to the fifteenth century, when the region from Spain to Iraq became Arabic speaking, in contrast to the Persian and Turkish-speaking areas to the east. In the Ottoman age (sixteenth to eighteenth century), all the Arab countries except Morocco, the Sudan, and parts of Arabia were ruled by a Turkish-speaking elite, but Arab culture continued to grow, esteemed both by the Ottomans and the Arabs. Major changes occurred in the eighteenth century in relations between the Ottoman culture and the provinces and between the empire and Europe. In the age of European empires (1800–1939), the dominance of Europe ended the possibility that "Muslim states and societies could . . . live in a stable and self-sufficient system of inherited culture" and required them "to generate the strength to survive in a world dominated by others" (p. 263). Reform was the preoccupation of governments and intellectuals alike, but the spread of European imperialist rule was virtually complete. In the present period, the age of nation states (since 1939), the imperialist regimes have been replaced by governments "committed to the cluster of ideas which

had gathered around that of nationalism: the development of national resources, popular education and the emancipation of women" (p. 351). It has been a period of population growth, urbanization, new forms of social stratification, and new mass media.

The historical experience is all-inclusive to Hourani: politics, society, religion, literature, thought, economics, architecture, art, and their interrelations are his subject. Narrative political history is replaced by succinct expositions of political conditions. In the treatment of the pre-Ottoman periods, the contributions of Julius Wellhausen, Ignaz Goldziher, Henri Lammens, and others—Orientalists one and all—still dominate, but of the five periods, that of the eleventh to the fifteenth century is allotted the largest number of pages, whereas the founding fathers of Orientalism and Arab nationalist historians and ideologues, who usually speak unkindly of Orientalists, consider it to have been a period of decline. Recent advances in urban history, anthropology, economics, and women's studies have also left their mark in this book. Specialists will differ, however, with some of the analyses. Especially troublesome are the discussions of the nature of Arab nationalism, its social roots and ideology, and its relation to Islam. With some ambiguity, Hourani's exposition follows some common notions that rest on fragile foundations. The analysis of the political process since 1939 is of a similar nature. Again, the presentation is ambiguous, but the thrust is that Ba'thism and Nasserism were movements of the disadvantaged orders; their military connections are casually mentioned, but nowhere discussed.

The book is clearly written, suitable for the beginner as well as the expert, but occasionally a beginner may need a bit more information about some particular. The exposition is based on an extensive knowledge of the sources and mastery of an extraordinarily large body of literature, scholarly and otherwise. This is a much-needed and admirable review of the state of the art.

C. ERNEST DAWN
University of Illinois,
Urbana-Champaign

SURAIYA FAROQHI. *Men of Modest Substance: House Owners and House Property in Seventeenth-Century Ankara and Kayseri*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1987. Pp. xxi, 268. \$49.50.

Suraiya Faroqhi's book responds to the question whether there was social and economic change in Ottoman Anatolian towns between the seventeenth and nineteenth centuries. Did the settlements on the plateau have an early modern history, or did they fail to break free from the timeless qualities some scholars associate with the phrase "the Islamic city"? This study, a companion to Faroqhi's *Towns and Townsmen*

of Ottoman Anatolia: Trade, Crafts, and Food Production in an Urban Setting (1984), uses the records of the Muslim courts of Ankara and Kayseri to sample the concerns of city dwellers at the turn, and the end, of the seventeenth century.

These court records have become increasingly popular among scholars eager to obtain large amounts of material in a more or less uniform format from periods and areas poorly reflected in Istanbul collections of tax registers. First used to good effect by Halit Ongan (*Ankara'nın 1 Numaralı Şer'iye Sicili* [1958]), the Muslim court decisions became the major source for a series of important articles (centered on Kayseri) by Ronald Jennings (beginning with "Women in Early 17th Century Ottoman Judicial Records—The Sharia Court of Anatolian Kayseri," *Journal of the Economic and Social History of the Orient* 18 [1975], 53–114) and an important book on life in Jerusalem by Amnon Cohen (*Jewish Life under Islam* [1984]). Faroqi takes their analyses a step further, using a variety of techniques to derive indices of behavior and change. For example, borrowing a method from David Herlihy's landmark article, "Church Property on the European Continent, 701–1208" (*Speculum* 36 [1961], 81–105), she can more accurately determine whether a given town quarter was likely to have a Muslim, non-Muslim, or cosmopolitan population. She compares the sparse descriptions of houses in the court records with modern studies of "traditional" houses in order to delineate the changeability of "tradition" and taste in housing. Her use of the materials is subtle, judicious, and far more suggestive than declaratory. Conclusions are always accompanied by suggestions for further research and for other tests that might clinch or alter the point at issue. There is a refreshing sense of limits here, born from a realization that the sample of towns, court records, and generations surveyed may not be sufficient. Faroqi is to be congratulated on her restraint and respect for the complexities of the evidence.

Faroqi believes "firmly, if cautiously," that Anatolia, after the whirlwind of the Celali revolts at the end of the sixteenth century, enjoyed a lengthy "Indian summer of urban life" (p. 22) and that active developments rather than a continued siesta marked the history of Ankara and Kayseri. A number of tables present her data in summary form, organized to reveal much that is fascinating: for example, changes in distribution of wealth involving those who served the Ottoman administration, non-Muslims, and Muslim taxpayers; the developing composition of town quarters as a function of the basis of urban wealth; confirmation of the role of the religious judge as a protector of women (and, on occasion, of non-Muslims).

A hallmark of Faroqi's scholarship is her interest in applying methods from neighboring (or even distant) fields. In this book she has used a number of ideas employed in the study of medieval Europe, but she has gone farther and derived a number of unex-

pected correlations. Medievalists using material from elsewhere in the Mediterranean will find much of interest in this excellent study, which deserves a wide readership.

RUDI PAUL LINDNER
University of Michigan,
Ann Arbor

MARY C. WILSON. *King Abdullah, Britain, and the Making of Jordan*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1988. Pp. xxii, 289. \$42.50.

Mary C. Wilson's book is an extensively researched history of the creation and evolution of Jordan from its inception following World War I to the death of its first monarch, Abdullah, in 1951. Although Great Britain's role as midwife in the conception of Jordan is well known, Wilson admirably describes the convoluted reasoning behind both Britain's postwar policies toward its newly acquired Arab empire and its often stormy relationship with the Hashemite family, particularly Abdullah. In addition, Wilson places Abdullah's role firmly within the context of inter-Arab politics and rivalries.

Wilson has used a multitude of primary materials, including Arab, Israeli, British, and American documents; she also conducted numerous interviews with leading Jordanian notables. Although the study includes useful maps, an index, and a thorough bibliography, an appendix giving a genealogy of the Hashemite dynasties would have been a welcome addition. In regard to British political interests and the economic and social developments within Jordan, Wilson's study has been complemented and supplemented by the more recently published *Lawrence of Arabia* (1990) by Jeremy Wilson and *The History of the Hashemite Kingdom of Jordan*, volume 1, *The Creation and Development of Transjordan: 1920–1929* (1989) by Ma'an Abu Nowar, but these works in no way detract from the valuable contribution to the body of knowledge regarding the evolution of Jordan made by Wilson's research.

Wilson debunks as myth both Abdullah's reputed bedouin origins and the recently much-publicized notion that Jordan was an integral part of historic Palestine. As Wilson carefully documents, Transjordan was not viewed by anyone, especially the British, as part of Palestine. Some Zionists, however, and British officials like Herbert Samuel, the first High Commissioner to Palestine, favored attaching the territory to Palestine in order to increase the amount of land available for Jewish settlement.

Wilson's study is particularly valuable for its documentation of Abdullah's political ambitions, first for his father, Sherif Husayn, as king of the Arabs, and subsequently for himself as king of at least greater Syria. The narrative traces the extensive and often labyrinthine financial and land dealings between Ab-

dullah and a wide variety of Zionists. The material regarding the willingness of various Jordanians, including Abdullah, to sell and/or lease land to Zionist investors provides a useful compendium to similar studies on land acquisitions in Palestine, in particular Kenneth Stein's *The Land Question in Palestine, 1917–1939* (1984).

Given his political ambitions, Abdullah was, as Wilson emphasizes, willing to accept the partition of Palestine in order to incorporate the proposed Arab section into his own realm. Great Britain, having concluded that a Palestinian state was not in its best interests, viewed the partition plan as a suitable means of assuaging Abdullah's ambitions. Wilson's study makes clear that Abdullah's support for Palestinian and Arab nationalism was largely dependent on the extent to which such support furthered his own political and financial interests. As a result, Palestinians generally distrusted and frequently opposed Abdullah. In light of this convoluted history, Abdullah's assassination by a disgruntled Palestinian in 1951 comes as a surprise only in that it had not occurred earlier.

JANICE J. TERRY
Eastern Michigan University

JOEL BEININ. *Was the Red Flag Flying There? Marxist Politics and the Arab-Israeli Conflict in Egypt and Israel, 1948–1965*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xix, 317. Cloth \$40.00, paper \$12.95.

For those familiar with the Arab-Israeli conflict, this is a challenging and painful book to read. Joel Beinin selectively recounts history to support his admitted goal of revisionism and his desire to "reconceptualize the history of the Palestinian/Arab-Israeli conflict through the lens of the history of Marxist politics" (p. 1). Using events surrounding Israel's war of independence in 1948 as the "baseline," Beinin compares the development, experiences, and effectiveness of the Communist Party in Egypt, where he examines the movement's three major tendencies, and in Israel, where he studies the Communist Party of Israel (MAKI) and the Marxist-oriented MAPAM, the United Workers' Party. He identifies the advent of nationalism in both countries (Nasserism in Egypt, Zionism in Israel) as the major reason that these parties ultimately failed, despite the "correctness" of the internationalist and anti-imperialist positions they advocated.

A point that deserves further elaboration is mentioned only once, briefly, early in the book. The apparent ease of access to materials in the Israel State Archives and other public and private Israeli archives has enabled Israeli researchers to "decisively" debunk "many components of the Zionist and, to a lesser extent, Arab nationalist mythologies." Beinin, appearing to miss the significance, continues: "Unfor-

tunately, neither the Egyptian nor any other Arab government has made materials on the post-WW II period in its national archive available for historical research. As a consequence, the focus of revisionist historical work has been on Israel and its actions" (p. 7). If, as claimed, he is examining these communist groups "comparatively and relationally," he cannot objectively do so without equivalent access to research sources from both sides.

The author provides an interesting and reasonably accurate historical rendering of the communist movement in Egypt, despite his failure to mention that many of the same individuals who founded the Palestine Communist Party in 1919 were later instrumental in founding their Egyptian counterpart. There are, however, serious flaws in the sections on Israel. The author repeatedly ignores or deemphasizes Arab-initiated acts that preceded the Israeli action he condemns. Beginning with the government of David Ben-Gurion, we are told, Israel developed and pursued a policy of "politico-military activism" (p. 10), instigating border incidents and demonizing the Palestinian Arabs while effectively intimidating, terrorizing, and oppressing them with the aim of confiscating their lands and using their property to build the economic base of the new, imperialist state.

There are even times when Beinin appears naive, as when he comments that the Zionists and Palestinians "do not even agree on whether the territory in dispute is called Palestine or Eretz Israel" (p. 18). Obviously such a disagreement goes to the heart of this intractable dispute, wherein two peoples have laid claim to one land. Or, consider the comment: "The technical device of translating *Palestine* as *Eretz Israel* in the Hebrew Dailies, while formally correct, rhetorically obscured the transformation of a land with an Arab majority into one with a Jewish majority" (p. 50). "Eretz Israel," meaning the Land of Israel, is not a translation of "Palestine," a name imposed by conquerors and never recognized by the Jewish people. As for the claimed rhetorical transformation, there was a documented Jewish majority in the territory designated for the new Jewish state by the partition plan of 1947. Had Israel's Arab neighbors not invaded, the Arab majority in the remaining territory could have continued to use the name Palestine to designate the state they would have had. On a far more serious level, Beinin's tendency to select only sources that support his arguments sometimes leads to distortion. For example, my book (*The Communist Movement in Palestine and Israel: 1919–1984* [1985], 331) discussed MAKI's need to recant its initial support for Israel's Sinai operation (which began on October 29, 1956). I had cited an article in *Kol ha'am* (Voice of the People, the communist newspaper in Israel) that referred to "a police action against bloodthirsty marauders and the Pharaoh on the Nile." Arguing that the communists never supported the campaign, Beinin cites only a subsequent

Kol ha'am article that referred to the Sinai operation as "an imperialist collusion" (p. 195).

From the point of view of style, this work would have benefited from careful editing. Consider the following cumbersome sentence: "The principle [*sic*] issues in the Palestinian-Israeli conflict—the refugee question, the location of Israel's borders, the creation of a Palestinian state, the character of Israeli democracy—became defined by a mythologized account that demonized Palestinian opposition to Zionism and denied realities experienced and criticized by many Zionists themselves" (p. 64). Readers may question the grammatical usage of the oft-repeated reference to "a Jewish problematic," but the uninitiated will surely be at a loss at the mention of "Borochovism," referring (without explanation) to Dov Ber Borochov, the important Marxist intellectual who was considered a "Zionist deviationist."

Finally, a minor point to be corrected: The origin of May 1 as the international workers' holiday was not the 1886 Chicago Haymarket strike (p. 98). As Rosa Luxemburg explained (in *What are the Origins of May Day?* [1894]) the proletarian holiday began in Australia in 1856.

Despite these flaws, I believe the book merits reading, if only because it adds to the general debate.

SONDRA M. RUBENSTEIN
Hofstra University

AFRICA

JAN VANSINA. *Paths in the Rainforests: Toward a History of Political Tradition in Equatorial Africa*. Madison: University of Wisconsin Press. 1990. Pp. xx, 428. Cloth \$45.00, paper \$17.50.

Jan Vansina has made a signal contribution to the enterprise of reconstructing Africa's early history as history, full of movement and change. He presents a sweeping interpretive integration of the differing kinds of evidence that bear on the past few millennia in the great equatorial rainforest regions of Central Africa. Vansina pays a major portion of his attention to the past 1,000 years, in which the earlier equatorial Bantu tradition in all its regional variety began increasingly, in different ways in different areas, to face pressures and opportunities, both economic and social, that led to growth in societal scale and complexity, and to the evolution of new institutions and new cognitive understandings. But he does not neglect the deeper background to this history, giving extended coverage, necessarily in less detail, to the broad features of culture and the major lines of historical development across the region between 3000 B.C. and A.D. 1000. It is a truly outstanding effort, readable, subtle, and integrative in its interpretations, and comprehensive in scope.

Several major findings emerge from the book. On the general level, Vansina demonstrates the splendid human creativity of history in equatorial Africa. Al-

though sharing common roots in the earliest Bantu society of circa 3000 B.C., the peoples of the vast Congo basin evolved an immense variety of cultural, social, political, and economic institutions, practices, and philosophies out of their common past; and Vansina explores with nuance and telling detail the interplay of independent innovation and cross-cultural influences in creating this intricately varied world. A specific contribution of import is Vansina's fascinating reconstruction of the nature of early Bantu society—that it was composed socially of Houses, groupings of kin and putative kin under the leadership of "big men" whose dominant position depended on wealth and personality more than heredity, and that its residential units were independent villages, each an aggregate of several Houses with the "big man" of the senior House acting as headman for all. Descent was bilateral; lineages and clans did not yet exist. The matrilineality so often attributed to the early Bantu was not present, he claims, but instead can be argued to have arisen in some areas, and patrilineal descent in other areas, under the specific historical conditions of later times. And finally, Vansina has much to say of importance to general historical theory about the relative causal salience of material versus cognitive realities in shaping peoples' histories. The common deep historical roots and the often parallel, even if not identical, material circumstances, and yet quite distinctive courses of social and political development, among rainforest societies over the past millennium means for Vansina that the cognitive reality through which people respond to their world stands forth as a crucial and independent causal factor.

It needs to be understood that a great mass of information additional to what explicitly appears in the book undergirds and provides the wider context for its arguments and reconstructions. In the kinds of work both Vansina and I produce, one must necessarily choose to write history that is adequately but not exhaustively substantiated or else one's readers lose their historical direction in a thicket of technical demonstration. A fair sample of collateral and backup data has aptly been placed in appendices to the book. Nevertheless, two kinds of additions to the text might have increased the reader's comfort level. One would be, particularly in the early chapters, to cite more consistently the particular historically germane root words themselves rather than the catalogue numbers for them; the words themselves, after all, are what allow a reader to engage something human and historical. And second, small summary explanations of contemporary developments in the savannahs south of the rainforest would have been useful in allowing the reader to better count or discount the occasional intrusion of southern savannah people onto center stage.

One major matter to take issue with, had it been integral to Vansina's reconstruction, would be his acceptance of Western and Eastern Bantu as two

coordinate branches of the Bantu language group. Based on a flawed statistical reckoning of the relative closeness of relationship across the group, this classification does not, however, significantly enter into the history he portrays, and can more suitably be challenged elsewhere.

Vansina describes the book as constituting "but an agenda and stimulus for vigorous future studies" (p. 250). He is wrong. It is a seminal study, as those words imply, but it is also a substantive history that will long retain its usefulness.

CHRISTOPHER EHRET
University of California,
Los Angeles

RICHARD GRAY, *Black Christians and White Missionaries*. New Haven: Yale University Press. 1990. Pp. viii. 134. \$20.00.

These essays by Richard Gray are written in the serene confidence that the Christian gospel survives and transcends the limited understanding and perhaps questionable motives of both those who proclaim it and those who adopt it. Gray opposes the view that Christian evangelism in Africa was simply oppressive of, or irrelevant to, Africans.

The essays fall into two groups. In the first, Gray focuses on the mission of the Capuchins to the Kongo province of Soyo in the seventeenth century, when the Atlantic slave trade first took on massive proportions. Gray follows the efforts of a Brazilian of Kongo descent and certain Capuchins who persuaded the Holy Office to condemn the slave trade in 1686, only to see their success set aside by vested interests. All that remained in Soyo was a Capuchin campaign to prevent the sale of slaves to heretics.

Although at odds with the missionaries on this point, the rulers of Soyo, partly with the aid of the Capuchins, maintained their independence in the face of Portuguese attacks and engaged in strenuous diplomacy with the Dutch and the Vatican. Catholic Christianity in Soyo played much the same part in the lives of the rulers, the elite, and the common people as it did in European states at this time, except that the missionaries monopolized priestly functions and allowed no indigenous priesthood to develop. Gray dwells on the evidence of the increasing strength of Christian belief and practice among the people despite adverse conditions and forces.

During the eighteenth and nineteenth centuries, after the departure of the Capuchins, Christianity is usually said to have been absorbed into indigenous religion, but this point is disputed and remains obscure. Gray resumes his argument at the point where renewed missionary endeavor is associated with the imperial conquest of Africa. Kongo Christianity was now only one case among many, and missionaries no longer faced local rulers sovereign in their territories.

In the course of a masterfully succinct survey, Gray distinguishes between syncretism, defined as a deliberate mixing of religious traditions, and theological pluralism, in which a continuous dialogue takes place. He emphasizes African agency in both evangelism and the appropriation of Christian themes, including protest against colonial rule and racial discrimination. His examples range from religious movements begun by African prophets to the uncompromising Kairós document, drawn up by South African clergy, black and white, in 1985, condemning apartheid as the work of the Antichrist.

The "truth" of Christianity in Africa is in a sense a quantitative question: how much of it is real? Unless and until the same question is asked of Christianity in Europe, it cannot be legitimately posed. The answer can in any case only be a political one. Gray is surely right in his emphasis on communication: to assert a Christian or for that matter an Islamic identity is to open oneself up to the constant challenge of values and doctrines articulated by fellow adherents, which at the same time mediate one's participation in the world-wide system of forces transforming local communities. Christianity itself, embedded in history, is continuously transformed by it. Concrete and detailed cases support Gray's lucid account of this transformation in Africa.

WYATT MACGAFFEY
Haverford College

JANET J. EWALD, *Soldiers, Traders, and Slaves: State Formation and Economic Transformation in the Greater Nile Valley, 1700–1885*. Madison: University of Wisconsin Press. 1990. Pp. xiii. 270.

The best Ph.D. dissertations are often studies of small areas that illustrate large questions. When converting such a dissertation into a book, it often becomes necessary to develop broader questions. Janet J. Ewald's book is an effort to do this recasting of her thesis research into a wider context.

Ewald's dissertation was a study of Taqali, a kingdom that arose in the northern part of the Nuba hills south of Khartoum in the Sudan. For geographic reasons, the Nuba hills had long resisted both conquerors from outside and state builders rising within. In the eighteenth century, however, conditions were changing. Trade networks were extending to the south, bringing horses and weapons to those in the hills who could protect traders and provide trade goods. At the same time, the demand for slaves in the kingdoms of the plains led to increasing conflict and slave-raiding in the hills themselves. The kingdom of Taqali arose because it could both use the outsiders and protect the peoples of the hills. A mark of Taqali's success was that it was never conquered until the forces of the Mahdi invaded in 1884. In the middle of the nineteenth century, the trade frontier

moved from the Nuba hills to more lucrative areas farther south, leaving Taqali completely surrounded but still autonomous.

The book becomes most interesting where Ewald tries to set Taqali into the larger Nile Valley framework, compares it to other states in the larger system, and analyzes the relationship between land ownership and political power. In Egypt, Sennar, and Darfur, rulers used grants of land to create support and taxation to create resources. In Taqali, the local communities resisted the monarchy's effort to do this. The king (makk) controlled slave-worked domains that provided the court with resources, but he lacked reserves of land to make grants to traders or military underlings. His wealth came from the traders and from his own estates, but he controlled only a limited land area. We thus have the paradox of a monarchy restrained by internal opposition but successful in fending off the attacks of outsiders. Islam linked the court to both traders and to the rulers and herders of the plains, but it differentiated the court culture from the local communities, which remained true to traditional religious beliefs and practices.

This discussion is presented with great skill. Ewald writes well and her arguments are tight and well made. I would, however, make three rather limited criticisms. First, the sections on Taqali are often too sparse. It would have been interesting to see her spin out some of her arguments more fully, particularly since she has approached her limited data with a great deal of rigor. Second, the organization of the book involves a division into two separate parts. The first looks at Taqali, the second at the larger Nile Valley system. The most interesting parts of the second part are those where Taqali is either compared to other states or used to give some perspective on the larger system. It would have been better to have integrated these parts and to have maintained throughout a view from the Nuba hills. Finally, Ewald uses more Arabic and Taqali terms than are necessary. I doubt if all of these terms are untranslatable.

Ewald's study is a valuable contribution to an increasing body of literature on states that emerged on the slave-raiding frontiers of Africa. The Sudan was important as a supplier of slaves for both Egypt and the Arabian peninsula, but the expansion of slave-raiding took place during the same period that the Atlantic slave trade was transforming West African political structures. The networks created by the external trade and the conflicts that resulted from increasing centralization meant that most of the slaves were used within the region. Taqali both used the slave trade and limited its effects not because its rulers wished to do so, but because their success in defending the hills made it possible for subject peoples to maintain a cherished autonomy.

MARTIN A. KLEIN
University of Toronto

LARRY W. YARAK. *Asante and the Dutch, 1744–1873*. (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1990. Pp. xiv, 316. \$64.00.

The precolonial inland kingdom of Asante (in present-day Ghana), which has been studied as much as perhaps any other traditional African state, is the focus of Larry W. Yarak's book. As his title indicates, Yarak seeks initially to analyze the origins and character of the relationship between Asante and the Dutch representatives who were located on the coast from the early eighteenth century to the time of their withdrawal from the area in 1872. The book, however, is more than an examination of Asante-Dutch relations; it has much to offer on Asante's triple relationship with the Dutch as well as with its coastal ally state of Elmina, and it culminates in what emerges as Yarak's ultimate concern: a study in the application of Weberian analysis of patrimonial officialdom to Asante.

The volume is structured into three segments. Part 1 provides a detailed discussion of the career and residency at Elmina—the Elmina resident being responsible for Dutch affairs—of the senior Asante official, Kwadwo Akyampon (1822–32), on the grounds that his career offers a case study in the nature of both the Asante residency itself and that of the broader provincial/imperial administrative system of Asante. Yarak argues that the administrative duties of the officeholder were based as much on his personal qualities as on the perceived needs of the king and that Kwadwo Akyampon's career typifies Max Weber's categorization of tradition-based, rather than formal-rational, domination to authority. In part 2 Yarak provides an in-depth look at the principal features of Asante-Dutch relations, demonstrating that while commercial and strategic considerations were vital throughout, the relationship was quite variable. In this section the author also confronts the historical debate surrounding the exact nature and significance of the "kostgeld," the annual payment made by the Dutch to the Asante king in acknowledgement of Asante's sovereignty over the lands on which the Dutch forts stood, and he discusses the conflicting Asante and Dutch interpretations ranging from rent/groundrent to stipend and tribute. In part 3 Yarak discusses the historical development of Asante's administrative mechanisms for conducting its political and mercantile relations with the Dutch and Elmina, pursuing his analysis of issues such as the nature of functions entrusted to residents, their status, and the increasing use of specialized envoys, along lines that substantiate his view of the Asante administration as befitting Weber's characterization of patrimony.

The work has several strengths. Yarak brings to his study considerable Dutch archival material and in fact applies Dutch sources to analyses of the workings of the Asante administrative system more extensively

than any scholar before him. His study expands our understanding of the Asante central government, its provincial administration, and its external relations. Above all, the author's position on Weberian patriarchal authority is intended to make a powerful statement in Asante (and indeed African) historiographical studies. He challenges Ivor Wilks's view (*Asante in the Nineteenth Century: The Structure and Evolution of a Political Order* [1975]) of Asante bureaucratic efficiency, professionalism, and "modernization" as deriving from formal relationships and presents the contrary position as emanating from the kings' efforts to bolster their own authority in the political hierarchy through controlling personal relationships over dependent officials, that is, Weberian patrimony.

A number of problems, however, arise out of the nature of the tasks the author sets for himself, his use of sources, and the model he constructs. Asante-Dutch/Asante-Elmina-Dutch relations and the nature of the Asante administration each constitutes a legitimate, full-scale subject of inquiry. Yarak is torn between the two and in the end fails to do full justice to either. For instance, he offers no comparative career analysis for Kwadwo Akyampon. As a result the reader is left with no option but to take the author's word that this official's career was "typical." Besides, the author's heavy dependence on archival material presents an imbalance in the use of sources. Only two sparse references are made to Elmina oral sources, the author thus failing to use the wealth of orally based local sources, including families descended from Dutch officials, some of whom are known to additionally possess small collections of records. As it happens, oral sources are utilized primarily in reconstructing the stool history presented in one chapter rather than in corroborating documentary evidence on career profiles, thus creating the unfortunate appearance of "two sources" used for different purposes rather than integrated. Furthermore, while Weberian (and other Western) models have an obvious theoretical merit, their application as the overriding analytical tool, as in Yarak's study, overshadows a legitimate concern for local historians: the need for a truly internal Asante perspective. Indeed, a relevant question that could have been addressed is how the Asante themselves viewed the unfolding of their history/administrative system. Moreover, evidence from other studies in Asante's relations with other foreign powers such as England indicates that skills were equally as important a factor in appointments as loyalty (an essential criterion in the patrimony model). Finally, the author surprisingly encounters problems in place names and orthography. A person from Elmina (an Edinanyi) is not referred to as "an Elmina," nor is someone from Accra (a Ga) "an Accra" any more than an individual from Holland is known as "a Holland." While this may be overlooked as a minor error whose repeated occurrence in the text does not detract from the study except as a frequent source of irritation, that such a

problem should occur at all at this stage of African studies portrays a lack of sensitivity to the local culture. The rendering of an Elmina royal name, Kodwo Dziewu, for instance, as "Kwadwo Dsiewu," situates the first and last names in two different foreign ethnic groups, although this evidently was unintended.

These weaknesses notwithstanding, Yarak has produced an important, stimulating, and provocative work of relevance not only to Asante studies but to analyses of other traditional political institutions throughout Africa as well.

JOSEPH K. ADJAYE
University of Pittsburgh

FRED MORTON. *Children of Ham: Freed Slaves and Fugitive Slaves on the Kenya Coast, 1873 to 1907*. (African Modernization and Development Series.) Boulder, Colo.: Westview. 1990. Pp. xix, 241. \$33.95.

The second half of the nineteenth century brought profound changes to the system of plantation slavery that Arab and Afro-Arab slaveholders had developed on the coast of present-day Kenya. The expansion of British influence on the East African littoral forced the abolition of the external slave trade in the 1870s, although slavery itself did not end officially until 1907. During the same period the growth of Christian missionary activity along the Kenya coast and the withdrawal of the Oromo peoples who had previously dominated the coastal hinterland created a tentative legal and geographical harbor for freed and escaped slaves. The struggles of these former slaves to defend their limited autonomy is the subject of Fred Morton's thoroughly researched and moving book.

The interruption of the slave trade stimulated an expansion of local slavery, but at the same time numerous and sizeable maroon communities challenged slaveholder dominance. Some escaped slaves formed independent, albeit short-lived, settlements in the areas previously dominated by the Oromo, while others attached themselves to established local rulers. Either way, the maroons survived brutal campaigns of raiding and involvement in the slave trade. Unfortunately, most of the experience of these clandestine communities remains obscure.

More thoroughly documented are the activities of the Bombay Africans, women and men taken from slave ships on the Indian Ocean and educated as Christians by British missionaries in India. Beginning in the 1860s some of these ex-slaves were brought to the Kenya coast, where they settled in Frere Town outside Mombasa or became involved in evangelical work in the hinterland. Animosity governed many of their relationships with white missionaries and local Muslim authorities. In Frere Town itself the missionary regime relied on floggings and confinement in stocks to enforce discipline and morality. But missionary notions of morality encompassed an accommoda-

tion with slavery on the coast that outraged the former slaves. The uncompromising refusal of Christian ex-slaves to return escapees to their masters brought them into repeated conflict with the local Muslim elite and with British representatives. Christian ex-slaves established agricultural settlements such as Fuladoyo and attached themselves to mission stations. The former slave William Jones rose to head the Rabai mission in the 1880s and courageously concealed hundreds of runaway slaves who sought refuge there. But the white missionary leadership scarcely objected when Fuladoyo was repeatedly attacked, and it removed Jones from his position when his actions became known. The advent of British rule in the 1880s and the British takeover in the 1890s brought little change, as white officials saw the support of local slave owners as crucial.

Morton argues that his account of escaped slaves should revise previous interpretations of slavery in East Africa. The presence of thousands of escaped slaves on the periphery of the plantation region was, in Morton's somewhat self-contradictory view, both evidence of the cruelty of coastal slavery and an ameliorating influence on it. He takes particular aim at historians, notably Frederick Cooper, whose works, Morton claims, characterize slavery on the Kenya coast as benign. Morton argues that Cooper and others have generalized about slavery from the record of the late nineteenth century, a period in which Morton asserts the treatment of slaves was improving. But the author's evidence for such a change is thin. Moreover, his tendentious criticism of the historiography of the Kenya coast moves well beyond the documentation his book provides and in the process obscures the complementarity between Cooper's work and his own. Nevertheless, the information that Morton brings to light about the role of escaped slaves in the decline of slavery adds substantially to our knowledge of the history of the Kenya coast and raises important questions for future research on the transition from slave to dependent labor in the region.

CHARLES AMBLER
University of Texas,
El Paso

ASIA

PHILIP C. C. HUANG. *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988*. Stanford: Stanford University Press. 1990. Pp. xiii, 421. Cloth \$49.50, paper \$16.95.

Once again Philip C. C. Huang has written an ambitious book about Chinese rural society. Once again he looks at the operation of the family farm as a key for understanding that history and characterizes its experience as a process in "involution," defined as a pattern of agrarian economic change "in which the total output expands, but at the cost of diminished

marginal returns per workday" (p. 11). Unlike his previous study (*The Peasant Economy and Social Change in North China* [1983]) that deals with the north China plain, however, this book focuses on the much more commercialized lower Yangzi. Moreover, he has extended his interest into the post-1949 era, taking in his stride changes under various phases of socialism.

Some parts of the argument are straightforward enough. Let us ignore the irritating references to Adam Smith, Karl Marx, and even A. V. Chayanov that Huang cites frequently to prop up his argument; no one would disagree with the central position of the family as an economic unit in premodern Chinese agriculture and by no stretch of the imagination would one assume that Smith could have believed that a model of perfect competition might apply to land transactions. Huang's argument in the pre-1949 half of the book is that, given the rather well-known high population density of the lower Yangzi, the predominance of small farms relative to the north, and the close involvement of these farms in the production of cash crops and handicrafts, a much lower proportion of farm households hired out their labor on a long-term basis than in the north. He explains this phenomenon in terms of the higher wages paid for male laborers, in response to which farm households put their women and children to work. This work, however, when rent and tax had been taken care of, produced diminishing marginal returns that left little above subsistence. This was the process of involution, and, one might add, the cause of the lower Yangzi peasant's continuous poverty.

This is a have-your-cake-and-eat-it-too argument that is hard to accept. If wages were going up (and presumably the argument refers to real wages), then the standard of living should rise, not fall. Huang says as much himself in discussing a passage quoted from a seventeenth-century text (p. 65). If by the twentieth century involution had set in, by virtue of the same argument, wages should fall and we should see a surplus of male laborers in the countryside and plenty of hiring out again. The evidence is less than definite on this point, and Huang's use of it does not make the task easy for the reader. Essentially, we have sporadic references from traditional sources, questionnaires from the 1920s to 1930s, community studies during the war years from 1941 to 1945, and a few surveys from the early 1950s. The few passages quoted from traditional sources do not give us a sufficient impression of a continuous record, and the 1929–33 wage figures quoted in Table 4.3 (p. 65) that Huang relies on for support do not show that wages were much higher in the lower Yangzi than in the north.

Space precludes more examples, many of which come in crucial places in Huang's argument. The interested reader will have to work out how best the yield figures cited in Table 5.3 (p. 89) can be used as evidence on productivity over the centuries, or how the relative prices of cocoons and rice in 1939 after

the silk market had collapsed (p. 126) could possibly be extrapolated to earlier years. It also does not help that all that the reader is told about Xu Xinwu's figures for Table 7.2 (p. 138) is that they are based on "decades of research" (p. 137). They are not the only figures available, and the one published paper I have examined based on this research is seriously flawed.

The focus shifts somewhat in the second half of the book from the whole of the lower Yangzi to Songjiang county near Shanghai. Between 1983 and 1988 Huang conducted 101 interview sessions with villagers at Huangyangqiao. He is able, therefore, to combine their reports with questionable brigade and county statistics for his historical reconstruction. Again, his observations are not nearly as radical as he wants them to appear. It is known that family production persisted despite the periodic extremes of the state, that state policies under collectivization often pushed peasant production beyond the bounds of profitable returns, and that it was economic diversification, including industrialization, that would raise the standard of living. Again the thrust of his argument, however, does not stand up to his data. Socialism, Huang tells us, has to be distinguished from the extremities of the Great Leap Forward and the Cultural Revolution. Socialism, Huang insists, introduced new agricultural technologies, coordinated water control, and brought about collectivization. The benefits of collectivization are debatable, but why must one assume that the market could not have produced the other changes, had that not been restricted by socialism?

DAVID FAURE
University of Oxford

PHILIP A. KUHN. *Soulstealers: The Chinese Sorcery Scare of 1768*. Cambridge: Harvard University Press. 1990. Pp. xi, 299. \$29.95.

Students of traditional Chinese history should be familiar with the Ch'ing or Manchu dynasty (1644–1912), which has left indelible marks on modern China. Noted for its peace, prosperity, and strength, the Ch'ien-lung reign (1736–95), especially its first half, witnessed the height of Ch'ing splendor. But beneath this dazzling surface there was a different world, as disclosed by Philip A. Kuhn. The sorcery panic of 1768 is a case in point.

The story of soulstealing in 1768 was brought to light as early as 1930 by the National Palace Museum in a series of publications of select Ch'ing documents. Not until half a century later, however, has it become the topic of a major work. Kuhn is the first scholar to see the significance of the story and has devoted many years to it.

Soulstealing was a magic art with a long history, possibly originating in South China. Equipped with this art, many believed, sorcerers could steal the souls from living persons by use of their names or hair.

The sorcerers might then change the stolen souls into powerful, magical agents for whatever purposes they saw fit. With their souls stolen, the people would fall sick or die.

Kuhn's soulstealing story begins in Te-ch'ing, a city near Hangchow, Chekiang province, around March 1768, where a floodgate project was under way. It was rumored that the crew needed the help of magical force to complete its job. The story, although baseless, spread like an epidemic over several provinces, including the metropolitan area, and it became very complex. Now it had many variants, each having several layers. Afraid, many people suspected vulnerable strangers—in particular, beggars and traveling monks—of being soulstealers and abused them. The frenzy soon seized the Ch'ien-lung emperor, who interpreted it in political terms. Under imperial pressure, provincial and local administrators apprehended and tortured suspects. The soulstealer hunt came to an end in November 1768 when Ch'ien-lung finally realized that there had been no soulstealing.

Kuhn is blessed with the ability to summarize the complex stories with clarity, freshness, and relevance. Each story is amply documented, well analyzed, and neatly presented. As a result, the book is not only a fine piece of scholarship but also a lively historical detective work. More important, it uncovers many social data hitherto buried in archives. For example, his description of the soulstealing hysteria has an allegorical quality, testifying to the existence of a neglected world of whispers, frustration, and insecurity that gripped the common folk. His accounts of the arrests, tortures, and coerced confessions of innocent people eloquently speak of the selfishness and insensibility of government officials. Even though the author does not claim it to be a social history, the book is a fascinating study of eighteenth-century Chinese society.

Moreover, the book contains a wealth of political information helpful to the understanding of the Ch'ing government. Its narrative of the Ch'ien-lung emperor's response to soulstealing is vivid and interesting. He vehemently reacted to sorcery because he considered it a mask of sedition. His overreaction further complicated the situation. The confidential memorials, as well as the court letters, were efficient means of direct communication between ruler and provincial officials. They also served as effective instruments of imperial control over the bureaucrats. Part of chapter 4 explores the legal aspect of sorcery, focusing on the Ch'ing code. With so much social and political information, the book casts light far beyond the soulstealing scare of 1768.

Kuhn's study draws on a variety of primary sources, most of which are archival materials. It is an informative and original work. Its value is also enhanced by twenty-one maps and illustrations. Solidly based and meticulously researched, the book is an important contribution to the field of Chinese history.

However good the book, the reader is tempted to

ask a few questions. For instance, the author's view of the Ch'ing bureaucracy seems debatable. Because of its "self-serving and routine-ridden habits" (p. 225), Kuhn argues, the Ch'ing bureaucracy during the eighteenth century was able to obstruct the monarch's campaign against the soulstealers. Although individual bureaucrats might be punished by the emperor, as a group they were well-entrenched and hard to break. Therefore, Kuhn believes, the soulstealing fears offered Ch'ien-lung an opportunity to attain monarchic control over the bureaucracy by "autocratic, unpredictable power" (p. 225) in addition to routine measures. Obviously, Kuhn has overstretched his interpretation.

Historically, the traditional Chinese bureaucracy had been progressively weakened by autocratic rulers since the reign of Wu-ti (140–87 B.C.) of the Former Han dynasty. Under the repeated imperial assault, for example, the premier (*ch'eng-hsiang*), once the policy maker of state affairs in the central government, gradually lost his power to imperial confidants and his office was finally abolished in 1380. The grand administrator (*chun-shou* or *t'ai-shou*), was originally in charge of the commandery, roughly a province, with the greatest number of powers over local affairs. Since his power frequently was undercut, he became a third-level administrator in the provincial bureaucracy. Moreover, the traditional Chinese bureaucracy was also enfeebled by division among its members. During Ming-Ch'ing times—the peak of China's autocracy—the bureaucracy was too weak to obstruct any monarchic campaign. In other words, Ch'ien-lung did not need the soulstealing panic to mobilize and control the bureaucrats.

Some of the author's statements are vague or even incorrect. For example, the Ch'ing confidential communication system was not introduced by the Yung-cheng Emperor (p. 49). Nor had Yung-cheng "faced direct challenges to his personal security" (p. 51). Translation of concepts from one culture to another is a difficult job. For example, the term "*Man-chou shih-p'u*" (p. 69) could mean "hereditary Manchu servants." The phrase "*ts'ai-sheng che-ko*" (pp. 85, 88) means "to take the fetus out of a pregnant woman and mutilate it." The two terms "*yu ch'u-hsi*" and "*ming-pai*" (p. 205) suggest "promising [future career]" and "clear-headed," respectively.

Despite the above minor issues, Kuhn's work is a readable and revealing study, desperately needed in the field.

PEI HUANG
Youngstown State University

FRANK H. H. KING *et al.* *The History of the Hongkong and Shanghai Banking Corporation. Volume 1, The Hongkong Bank in Late Imperial China, 1864–1902: On An Even Keel.* New York: Cambridge University Press. 1987. Pp. xl, 701.

The Hongkong and Shanghai Banking Corporation, one of the premier multinational banking groups not only of the Far East but also globally, is to be complimented for opening up its well-preserved archives for a wide-ranging, four-volume study of its operations from 1864 to 1984. Its choice of Frank H. H. King and his associates to conduct this massive effort is equally praiseworthy, for King provides rigorous academic standards as well as familiarity with the "Eastern banking" field from the perspective of the West. Such a perspective will contribute to a more rounded picture because recent studies on the financial developments of late-imperial China, such as K. C. Liu's ("Credit Facilities in China's Early Industrialization," in *Modern Chinese Economic History*, eds. Chi-ming Hou and Tzong-shian Yu [1979]) on the credit crunch of 1883–84 and M.-C. Bergère's (*Une crise financière à Shanghai à la fin de l'ancien régime* [1964]) on the banking crisis of 1910, have relied more on sources from the Chinese side. All in all, this new history of the bank is a great advancement over its earlier effort, when it commissioned fiction writer Maurice Collis to produce the one-volume *Wayfoong: The Hongkong and Shanghai Banking Corporation* as part of its centenary celebration in 1965.

The volume under review traces the bank's uncertain beginning, its near collapse in 1874–75, and its successful expansion under Thomas Jackson through 1902. It shows that operating a Western bank in the nineteenth-century Far East, despite the West's overwhelming financial resources and political power in that region, was no easy task. Indeed, several of the older and larger banks such as the Oriental Bank failed. That the Hongkong Bank was able to overcome difficulties and rise to greatness was due to several of its unique features and a small coterie of capable and long-lasting managers.

The original missions of the bank were to be a regional exchange bank and a commercial bank financing the trade between the China coast and Europe or North America. It set up its two main offices in Hong Kong and Shanghai. Since Hong Kong as a crown colony was able to provide a charter under British colonial banking regulations, it became the locale for the bank's chief manager and board of directors and thus the only major Western bank whose headquarters was on the China coast. This had several consequences. The bank became a depository of British treasury and colonial funds in Asia, had special rights to issue Hong Kong bank notes, and, through its continuous presence (first on the coast, followed by an agency in Peking) maintained its long-term effort to cultivate friendly relations with Chinese officials. As a result, the bank became China's foremost merchant banker. During the period covered by this first volume, most of the Chinese government loans were arranged through the Hongkong Bank, with their commissions making up the largest part of the bank's profits.

China scholars have generally been critical of the

Western bankers' role in aiding and in taking on themselves the exploitative capabilities of European powers in China. The view from the Hongkong Bank is quite different. King shows that, at least through 1902, the bank had little influence over British imperial policy. On the contrary, it often had to fend off excessive regulatory demands from the British treasury. King argues that the bank played fairly in the China loans, deftly adapted to Chinese ways of negotiation and setting up loan terms, while at the same time helping China gain access to the London capital market. He contends that the bank as a China-based, profit-making institution realized that its long-term interest lay with a China successful in achieving modern economic developments. As for the bank's culpability over China's several financial crises during this period, King does not address this issue directly. It appears that there was no evidence from the bank's archives to suggest willful intent or collusion with other Western banks when it withheld credit to Chinese banks at critical points. Instead, King shows how deeply the bank had become involved in the more risky business of financing the operations of Chinese tea brokers and processors in the interior, away from the relative security of the treaty ports (p. 505). Whenever a tightness of credit occurred in the market, the Hongkong Bank's recall of unsecured "chop" loans was, according to King, simply prudent business practice.

By oral interviews and from the archives, King also provides a fascinating account of the various people who staffed the Hongkong Bank: their social background and family connections, the professional training and requisite service in the London office prior to posting to the East, the rigid hierarchy that existed among the "Eastern staff" (Europeans hired from London), the Asian Portuguese staff hired locally, and the Chinese staff (most of whom served under the Chinese compradore). The Chinese compradore as an institution within the bank lasted well into the mid-twentieth century and provided the bank (as it did other British or American companies) with the rationale that there was no need for them to learn Chinese or to deal directly with their Chinese counterparts. After all, according to this view, the treaties that the West forced on the Chinese gave China the right to shut foreigners out, holding them largely to the restricted confines of the treaty ports. But it did not deter Japanese companies such as the Mitsui Company from training their employees in Chinese, so that by the early twentieth century the Japanese were able to do without Chinese compradores, send their own sales representatives into the interior, and thus begin to outperform the Europeans. Besides being a sound financial and institutional history, this volume also offers insights into the social mores and cultural blinders of Europeans in Asia.

WELLINGTON K. K. CHAN
Occidental College

PAUL J. BAILEY. *Reform the People: Changing Attitudes towards Popular Education in Early Twentieth-Century China*. Vancouver: University of British Columbia Press. 1990. Pp. x, 296. \$49.95.

Paul J. Bailey has carefully traced educational debate in China from the late 1890s until the New Culture Movement. Employing both Chinese primary sources and Japanese and Western monographs, Bailey depicts evolving attitudes toward popular education and its goals during these two decades. Crucial to the history of popular education was the change in government educational goals from training an official elite to nurturing a general citizenry. This change, which Bailey situates around 1900, derived from the belief that national wealth and strength depended on the qualities of the populace. Here Bailey signals a theme that permeates all Chinese educational programs during the twentieth century: an extraordinary faith in education's power to effect positive change and transform the people.

Other noteworthy findings emerge. Bailey contends that educational continuities extended from the Qing across the revolution of 1911 into the republic, as illustrated by the carry-over of personnel into the republic's educational administration and professional associations, the steady diminution of the place of Confucian classics, and the methods and purposes of popular education. More revisionist is his argument for the continued influence of Japanese educational models rather than displacement by American structure and concepts. Bailey also maintains that Western influence was more ambiguous than generally depicted. In addition to America's citizenship education and John Dewey's pragmatism, the centralism, paternalism, and efficiency of German education found favor.

Illustrating continuity, Bailey asserts that educators were recommending use of the vernacular much earlier than the May Fourth Movement, advocating vocational training more than a decade before the Chinese Vocational Educational Association was founded in 1917, and commending literacy classes and public lectures before James Yen's Mass Education Movement and the YMCA's popular educational programs. Bailey thus joins other scholars in denying that the New Culture Movement marked a revolutionary departure, while his documentation of its roots in late Qing further elucidates the genesis of change in modern China.

Even so, some of the evidence with which Bailey documents his thesis engenders a sense of unease. Vocational education is one case in point. Bailey quotes Qing reformers who stressed the importance of vocational education and notes that the dynasty issued regulations on vocational education in 1904 and 1907 and urged provinces to found vocational schools. Bailey's statistics, however, reveal a total of seven higher-level vocational schools in 1907 and forty-six lower-level schools for all China; although

some growth had occurred by 1909, no institution reported an enrollment of 300. Not very convincing testimony to a commitment to vocational education.

Evaluating writings in order to deduce attitudes is tricky. To what extent do educational blueprints represent an ideal, much of which admittedly cannot and will not be implemented? To what extent are the blueprints and essays influenced by current fads and popular tenets? To what extent, that is, can they be interpreted as an accurate reflection of attitudes and priorities? Essays, models, and government plans do not equal a movement. The roots of literacy campaigns, vocational programs, and usage of the vernacular may be found in early educational debates, but commitment leading to actualization awaited the New Culture Movement. Bailey has not been sufficiently cognizant of the old saw: "Actions speak louder than words." For an accurate assessment even of attitudes toward popular education in early twentieth-century China, Bailey's study would benefit from greater attention to implementation. The work, nevertheless, amply illustrates the wide range of Chinese educational theory during the early twentieth century and affirms that Chinese educators were already well acquainted with precepts and practice in Japan, the United States, and Europe.

JESSIE G. LUTZ
Rutgers University,
New Brunswick

PETER ZARROW. *Anarchism and Chinese Political Culture*. (Studies of the East Asian Institute.) New York: Columbia University Press. 1990. Pp. xi, 343. \$39.00.

Peter Zarrow's extensively researched study of Chinese anarchism contains a comprehensive presentation of the principal ideas held by both key thinkers of the first generation (Liu Shipei, Wu Zhihui, and Li Shizeng in the 1900s) and by leading activists of the second generation (Liu Shifu and his followers in the 1910s). The bulk of the book consists of careful readings of several short-lived but nonetheless critical journals published by Chinese intellectuals sojourning in Tokyo and Paris, where they came into touch with figures such as Kotoku Shusui and Jean Grave, the editor of *Les Temps Nouveaux*. Research scholars will find a wealth of textual and biographical information otherwise only accessible in scattered sources and impenetrable texts. This book consequently must be read not only for its analytical conclusions but also as an invaluable reference for understanding the radical social criticism that shaped the iconoclastic New Culture and May Fourth movements.

Several features of the book deserve special attention. Instead of defining anarchism in China merely as the translation of Peter Kropotkin and Pierre-Joseph Proudhon into Chinese, Zarrow looks for the indigenous sources of a radical alternative social vision. He sees Chinese anarchism both as emerging

out of an eclectic espousal of translated Western ideologies and as a creative reformulation of salient features of traditional Chinese political thought (Confucian, Taoist, and even Buddhist). It was the anarchists, for instance, who reconstituted the moral philosophical discourse of the Neo-Confucianists into a modern social ideology and coined new phrases that permitted classical Chinese to convey non-classical ideas like "the masses" and "class struggle." This is certainly an important point that deserves serious attention. One only wishes that greater care had been paid to the translation of such key terms as *qiangquan* (variously rendered as "power," "authority," "force," and "coercion"), *liangxin* ("good-hearted"), *zhian* ("order"), or *minquan* ("human rights"). These translations are often loose and at times incorrect. They are unreliable as the foundation for the kind of analysis that the author attempts.

Zarrow argues for the significance of Chinese anarchism in terms both of its influence on mainstream modern Chinese political thinking and its contribution to the general development of anarchism as a social ideology. In China, he shows how Chinese anarchists repeatedly first stated revolutionary ideas that later appeared in socialist thought. Many features of China's "utopian Marxism" and Maoism, he argues, have precedents in anarchist writings. In the global context, Chinese anarchists such as Liu Shipei "strikingly foreshadowed" Lenin's notion of imperialism and national liberation struggles: "Thus the Chinese anarchists not only began the process of the signification of Marxism, but independently reached conclusions similar to those of European Marxists" (pp. 174-75).

In paying close attention to the intellectual formation of individual anarchists, the author is able to show how Chinese anarchism from its moment of inception involved the articulation of two strikingly divergent strains. The anarchists in Tokyo, grounded in the evidential scholarship of the Qing, looked back nostalgically to a preimperial Chinese past where they imagined a utopia of honest labor and equality that had nothing to do with machines or capitalists. The anarchists in Paris, by contrast, were energized by a faith in the benefit of Western science and technology and believed in the attainability of a society of ease and comfort for all at the end of the evolutionary scheme. Consequently, to speak, as the author does elsewhere, of Chinese anarchism as one system of ideas risks lending to its multiple expressions a coherence that may be misleading. In concentrating on the theoretical import of Chinese anarchism, furthermore, the author appears to have opted for a less than perfectly contextualized presentation of Chinese anarchism as a historical phenomenon.

One of the virtues of this impressive study is its suggestiveness for other modern Chinese intellectual historians, especially those concerned with contemporary issues involving concepts of freedom and human rights. It is difficult for any observer to deny that

China's modern political history has been shaped by the forces of nationalism and socialism. Impressed by the power of the fascist as well as the Maoist states, experts on Chinese political culture have generally stressed the deeply ingrained habit of subservience of the ordinary Chinese toward figures of authority. In the face of the largest standing army and the most highly centralized (though unraveling) party state in the world, what role can the undying anarchist aspiration for a noncoercive society of individual autonomy play in twentieth-century China? Zarrow does not answer these questions directly. But this pioneering work, which needs to be read for its own sake, certainly has much to contribute on these fundamental issues.

WEN-HSING YEH
University of California,
Berkeley

DENNIS L. MCNAMARA. *The Colonial Origins of Korean Enterprises, 1910–1945*. New York: Cambridge University Press. 1990. Pp. xiv, 208. \$44.50.

This book highlights a number of important issues concerning the origins and practices of Korean entrepreneurship in finance, commerce, and industry during the Japanese colonial period, and its legacies in post-colonial South Korea. Dennis L. McNamara argues succinctly that Korean entrepreneurs emerged as dependent rather than compradore capitalists in an autarchic alien state and formed an interdependent network of benign capitalists belonging to a small circle of aristocratic families and their associates.

Stating that Korean society had "long aristocratic traditions" (p. 33), the author sees the continuities in patterns between aristocratic agrarian elites of the Chosŏn dynasty and financial and industrial elites in the colonial period, although some nonaristocratic entrepreneurs became successful in commerce in the dynamics of colonial transition. Similarly, McNamara argues for an "inner circle" of major business leaders during the First Republic, many of whom were capitalists dependent on colonial rule (p. 127). He concludes that a distinctive Korean style of entrepreneurship is characterized by "networks among kins and close circles of associates," which proved to be necessary to limit risks and insured some continuity of investment and direction (pp. 135–36). Korean entrepreneurs displayed "innovative skills in organization, development of markets, and application of technology" despite an oppressive colonial state "primarily concerned with military security and economic productivity" (p. 35).

McNamara examines only three important family-controlled business groups or *chaebŏl*, led by Min Tae-sik in finance, Pak Hŭng-sik in commerce, and Kim Yŏn-su in industry. The author attempts to provide in the first four chapters both theoretical and

historical frameworks in which these *chaebŏl* emerged. He asks (p. 11) the important question, "How does a local business elite survive, much less prosper, in the shadow of a well-financed colonial state with absolute authority?" He finds "Korean colonial business ties with the Japanese less amenable to the heuristic distinction between collaboration and patriotism" (p. 11). He finds it useful, moreover, to distinguish "comprador capitalists" who invest their talents and capital in foreign enterprises, such as Han Sang-yong, from "dependent capitalists" who, by contrast, own and manage their local enterprises, such as the three families examined here (p. 11).

McNamara emphasizes the continuity of aristocratic traditions in the rise of the Min family of Yohtŭg (*sic*) seat, as well as the Kim family, which he fails to identify by the clan seat, *pon'gwan*. He does not elaborate, however, on their aristocratic lineages and how they were related to other local entrepreneurs by kinship ties and marital relations. It is unfortunate that the term *yangban* was not used in referring to the ruling class of the Chosŏn dynasty and its aristocratic descendants in colonial Korea. It is regrettable that there are many misprinted Japanese names (for example, Saitō "Minoru" for Makoto, "Yanihara" for Yanaihara), and wrong Chinese characters in the glossary. Lastly there is a severe flaw in scholarship and editorial practice by not having acknowledged in the footnotes the pioneering work with a similar title by Carter Eckert, *The Colonial Origins of Korean Capitalism: The Koch'ang Kims and the Kyŏngsŏng Spinning and Weaving Company, 1876–1945* (Ph.D. dissertation, University of Washington. 1986; published as *Offspring of Empire: The Koch'ang Kims and the Colonial Origins of Korean Capitalism, 1876–1945* [1991]). After McNamara's book was published, errata were made separately in which the author acknowledged Eckert's contribution.

FUJIIA KAWASHIMA
Bowling Green State University

RONALD INDEN. *Imagining India*. Cambridge, Mass.: Basil Blackwell. 1990. Pp. vii, 299.

Critiques of Western knowledge of other societies have mounted since the publication of Edward Said's *Orientalism* (1978). Contributing to this burgeoning critique, Ronald Inden's aim is to prise open the academic discourse so as to restore the space for human agency erased by Indology's persistent assumption that India is composed of certain essences. Although the critique of Indology's essentialism is not new, the place and forms of essences privileged in South Asian scholarship has never been put to as comprehensive and learned a scrutiny as in this book.

The immense learning and analytical sharpness of the book is evident from the very first chapter, which ranges widely in analyzing the key philosophical assumptions and methods of Western discourses that

have framed India as Europe's Other. The following four chapters trace Indology's projection of caste, village India, the Hindu mind, and divine kingship as essence. While granting disagreements and discords within Indology as a discourse, the author argues that such differences concerned the weight given to different essences but not essentialism. Carefully analyzing one "hegemonic" text after another—those of Hegel and the German Romantics such as Friedrich von Schlegel and Max Müller, British utilitarians and empiricists such as James Mill and H. H. Risley, Indian nationalists and Marxists, and postcolonial Euro-American scholars—the author shows that the practice of making agents out of essences, or "substantializing" them as he calls it, has given us much of what we know about India. So powerful and enduring have been the epistemological procedures established by Indology that even much-acclaimed recent works on India, such as those of Louis Dumont on caste (*Homo Hierarchicus* [1970]) and Burton Stein on state and society in south India (*Peasant State and Society in Medieval South India* [1980]) have not escaped the long shadow of Indology. Rejecting the essentialism of nineteenth-century Indology and its twentieth-century successors, the author draws on R. G. Collingwood's concept of "scale of forms" to offer an alternative model of historical knowledge (*An Essay on Philosophical Method* [1933]). Chapter 6 employs this model to describe the imperial formation of the Rashtrakutas between 753 and 975 as a "scale of forms"; contrary to Indology's assumption, caste, urban and rural assemblies, temples, processions, and kingship do not appear here as embodiments of certain essences but as a constellation of multiple, overlapping, contending, and changing human practices.

While scholars may find the concept of "scale of forms" intriguing and useful, the book's critique of Indology is bound to elicit sharp disagreements. Even those sympathetic with its aims, as I am, will question Inden's exclusive focus on Indology's representations because this tends to present the Indological discourse as uniform, determined always by the governing logic of the Self-Other dialectic. Yet an analysis of Indology's enunciation would have shown that its historical functioning involved splits, contradictions, and ambivalences that the Self-Other dialectic could not entirely contain. The author himself depicts scholarship bearing this character, but his exclusive concern with Indology as a set of representations, determined by its origin in the Western will to power, means that he cannot provide a theoretical space for it. As a result his descriptions of challenges to dominant frameworks read more as episodic breaks in the career of Indological ideas and less as systematic effects of historical practices. This is ironic, given the value the author places on human agency. Such a criticism, however, should be taken as an indication of the sheer scope of questions raised by this book, for its learned and persistent critique of Indological

scholarship provokes us to rethink the ways in which historical knowledge is produced.

GYAN PRAKASH
Princeton University

THOMAS R. METCALF. *An Imperial Vision: Indian Architecture and Britain's Raj*. (Philip E. Lilienthal Imprint, number 2.) Berkeley and Los Angeles: University of California Press. 1989. Pp. xiv, 302; 15 plates. \$35.00.

G. H. R. TILLOTSON. *The Tradition of Indian Architecture: Continuity, Controversy, and Change since 1850*. New Haven: Yale University Press. 1989. Pp. viii, 166. \$25.00.

An observant traveler walking the streets of Rome, Istanbul, or Vienna can hardly fail to observe that architecture is not solely the mirror to the aesthetic of a particular elite, or even a matter of spacial organization; as often as not, it is a political statement. So too was it with the British Raj. British building activities in India were not, as is sometimes supposed, limited to the construction of barracks, law-courts and railroad stations, of magistrates' bungalows and hill-station retreats. Lord Wellesley's Government House in Calcutta (1799–1803), the Government House in Madras (1800–02) with its vast Banqueting Hall, and the princely residencies at Lucknow (1780–ca. 1800) and Hyderabad (1803) were classical structures emblematic of the order that India's European rulers sought to impose on their far-flung, seemingly chaotic dominions. So, too, in a different time and in a different stylistic mode, were the Victorian Gothic and Indo-Saracenic public buildings of Bombay; the boarding-schools (such as Mayo College), where Maharajas' sons were molded (up to a point) to be English gentlemen; the campuses (such as Aligarh College), where Western and indigenous values were somehow to be melded; and Lutyens's vision of an Anglo-Mughul New Delhi.

This is an important aspect of the British impact on the subcontinent, and it is ably addressed in these two scholarly monographs, which, with their quite disparate approaches, neatly complement each other. Each in itself is a major contribution to the growing literature on the architecture of British India, in which Sten Nilsson's *European Architecture in India, 1750–1850* (1968), Robert Irving's *Indian Summer: Lutyens, Baker, and Imperial Delhi* (1981), Jan Morris's *Stones of Empire: The Buildings of the Raj* (1983), Philip Davies's *Splendours of the Raj* (1985), and the contributions of Mildred Archer, have enabled the perspicacious traveler in India to view the monuments of the Raj with a new eye.

Thomas R. Metcalf's book focuses on the relationship between imperial power and the architectural landscape. As he writes in his preface, he is concerned with "the relationship between culture and power as expressed in architecture," and with "how political authority took shape in stone, and how, in turn, these

colonial buildings helped shape the discourse on empire of the later nineteenth century" (p. xi). He states that he has no interest in merely chronicling the architectural history of British India; rather, he shows how alien rulers, when confronted by an exotic and (to them) largely incomprehensible aesthetic tradition, reacted by confining themselves within the framework of their own prior experience—the classical in the late eighteenth and early nineteenth centuries; Victorian Gothic during the late nineteenth century. But even then, recognition of the complexities and ambiguities of ruling an Oriental empire resulted in the adoption of a revived Indo-Saracenic style, soon to be countered by a classical revival originating in Great Britain and then exported, as in the case of Herbert Baker in South Africa, throughout the empire. As Lutyens himself wrote in 1903 (Metcalf, p. 176), "in architecture Palladio is the game!"

But the changing Raj of the early twentieth century demanded a measure of recognition of the Indian component in the British-Indian empire. Hence, Lutyens, confronted with the challenge of willing a New Delhi into existence, endeavored to integrate Western and native elements. The result was (and is) magnificent in its way, but the overarching vision, "that of reestablishing the Raj on a new footing in the twentieth century," was fundamentally flawed (so Metcalf argues), since it ignored, in architecture as in politics, "the English-educated Indians, who as a 'modern' elite saw themselves as the natural successors of the Raj" (p. 240). Tenaciously, that vision clung to an earlier ideal of empire, in which a traditional India of daunting diversity needed a *Pax Britannica* to hold it together.

The focus of much of Metcalf's previous writing has been the impact of British rule on traditional North Indian rural society, exemplified by two definitive studies, *The Aftermath of Revolt* (1964) and *Land, Landlords and the British Raj, 1857–70* (1979). In the present book, working on a much broader canvas, he brilliantly analyzes the public buildings of the Raj as emblems of an evolving imperial ideology. G. H. R. Tillotson is primarily an architectural historian whose monograph, *The Rajput Palaces* (1987), was a major contribution to Indian art history, and whose *Mughal India* (1990) provided visitors to India with an admirable handbook of Mughal architecture. Unlike Metcalf, Tillotson is less concerned with the British architectural legacy as an expression of the ideology of empire than with its impact on traditional Indian craftsmanship and design. He defines his undertaking as "a study of the changes in India's architectural tradition and in Indian taste that occurred in response to the influence of British architecture in India and the policies of British imperial rule . . . [A] study of part of the transformation of one civilization by another, and of prolonged crises of power, responsibility and identity" (p. vii). In a way in which Metcalf's book was clearly not meant to be, this is in

part a highly compressed history of Indian architecture during the British period. The British presence inevitably weakened indigenous artistic tradition, elevating imported styles and construction methods to which, no less inevitably, some Indian patrons (among the Maharajas, in particular) were drawn. For their part, the majority of British were less than appreciative of the Indian aesthetic, although there were exceptions: S. S. Jacob in Rajasthan and F. S. Growse in Mathura and Bulandshahr (Uttar Pradesh), for example.

That a historian of Indian art should regret the dilution of indigenous tradition under alien pressures is natural enough, but the Indo-British relationship was a complex one, involving both selective assimilation and rejection. In fact, beyond his declared objectives, Tillotson has written a lively and scholarly contribution to the cultural history of modern India. Both books are well illustrated with photographs and line drawings, most of which will be unfamiliar to the nonspecialist reader.

GAVIN R. G. HAMBLY
University of Texas,
Dallas

CHRISTIANE HURTIG, *Les Maharajahs et la politique dans l'Inde contemporaine*. Introductions by JEAN LECA and JACQUES POUCHEPADASS. Paris: Presses de la Fondation nationale des sciences politiques. 1988. Pp. xxiv, 369. 220 fr.

Christiane Hurtig's insightful, multidisciplinary study of the Indian princes after independence seeks to destroy our cultural stereotypes about the roles of traditional elites in the modernization process. It emphasizes the importance of examining political activity within its historical and social contexts and is unusual in its sympathetic portrayal of the Indian princes as modernizing politicians.

The first half of the book analyzes the princely participation in electoral politics from the elections of 1951–52 to those of 1980 and the five-year (1967–71) effort to abolish the privy purses guaranteed to the princes in the Indian Constitution of 1950. Arguing that the princes had always been politically active, Hurtig meticulously describes the Congress Party's mobilization of erstwhile rulers as political candidates, particularly in areas where the Congress lacked a strong organization or in periods when its continuing ascendancy was threatened. Like other parties across the spectrum, it sought to capitalize on the value of princes as legitimizing and effective candidates.

Hurtig incisively asserts that the abolition of the privy purses did not lead to increased social or economic equality because new elites entrenched through electoral politics since independence had perpetuated new forms of inequality. Essentially, Indira Gandhi delivered the princes as sacrificial

lambs to her leftist supporters when she was being challenged by the old guard of her own party. Conceding that attenuation or outright abolition of the privy purses and other privileges was probably inevitable, Hurtig contends that the means used to obtain this end, namely constitutional amendments that established the superiority of Parliament over the judiciary in the achievement of the directives principle listed in the Constitution, were potentially dangerous to democratic institutions and practices. These constitutional changes made possible the state of emergency in 1975 and its denial of civil liberties.

The second half of this work discusses specific former rulers who were and are politically active either on the state or national level. In the former category are princes in Rajasthan, Madhya Pradesh, Gujarat, and Orissa. In the latter are Sriraj Meghrajji of Dhrangadhra, Karan Singh of Kashmir, Mohinder Kaur of Patiala, and Raja Dinesh Singh. Like Suzanne Rudolph and Lloyd Rudolph in their work on Gandhi (*The Modernity of Tradition* [1967]), Hurtig argues that when we move beyond positing modernity and tradition as binary opposites, individuals, such as Gandhi or princes, who are labeled as traditional because of external appearances or culturally defined ideas of tradition are indeed modernizers. Princely participation in electoral politics contributed their legitimacy to the new regime, mobilized underrepresented groups such as the scheduled castes into the electoral process, and helped to integrate the most conservative elements in their former states into the democratic system. The princes are also modern in their use of rational means of organization and communication, concerned about their self-interests but also imbued with a strong sense of service, and no more dysfunctional than other Indian politicians in such practices as shifting party affiliations. Given the continuing prominence of Vijaya Raje Scindia of Gwalior in the Bharatiya Janta Party and the ascendancy of her son, Madhavrao Scindia, among the younger generation of Congress leaders after the assassination of Rajiv Gandhi, these chapters have both historical and contemporary interest.

Besides Indian newspapers and government documents, this study is based on extensive interviews with princes, their administrative support staff, and other political leaders in India. These interviews are particularly valuable since some key rulers such as Karni Singh of Bikaner and Fateh Singh of Baroda are now dead, but the interviews unfortunately are not fully documented. There is a sizable bibliography, but it lacks some works, such as my own monograph (*The Princes of India in the Twilight of Empire* [1978]), which traces the efforts of a self-selected group of princes to participate in politics on an all-Indian scale under the British, that would extend the historical context of Hurtig's work. Apparently the manuscript was completed in the early 1980s since later works such as Vijaya Raje Scindia's as-told-to autobiography (*Princess* [1985]) are not listed. Still, this book is a most

welcome addition to the scholarly literature on the princely states.

BARBARA N. RAMUSACK
University of Cincinnati

ROBERT J. KING. *The Secret History of the Convict Colony: Alexandro Malaspina's Report on the British Settlement of New South Wales*. Boston: Allen and Unwin; distributed by Paul and Company, Concord, Mass. 1990. Pp. xii, 179. \$29.95.

When we were all younger and the world was a much simpler place, it was taken for granted that Australia had been founded as a prison. After 1775, convicts were accumulating in prison hulks at the rate of a thousand a year; New Zealanders ate people; and the southwest coast of Africa was found on inspection to be exceptionally uninviting. Thus, the new penal colony would be established at Botany Bay, where the natives were neither strong nor particularly hostile and smelled, at least, as if they ate fish rather than human beings.

In recent and more suspicious times there have been attempts to give grand motives to the founders. Some argue that the settlement of New South Wales was part of a general campaign to prevent the French from getting control of India. Others think that the English wanted from the beginning to take over the moribund Dutch and Spanish empires. And, of course, from the perspective of forty years, that is what happened. By 1830 Britain had a new empire, while the Dutch and Spanish had lost much of theirs. But had there been a grand strategy, a conscious plan of campaign, in the minds of men who mattered?

It may be that there was a grand strategy in the mind of James Matra, a Home Office employee, but questions remain about the extent of his influence. Now we learn that Alexandro Malaspina, a Parmesan in Spanish employ who visited New South Wales in 1793, thought that the English were up to something. His report was written when the Spanish were still smarting from the Nootka Sound dispute, and it was intended to promote Malaspina's career. Unfortunately he chose to join the Aranda faction at Court on his return to Madrid; he may even have tried to weaken Godoy's influence with the Queen. Godoy, who had conspiracy theories of his own, had Malaspina imprisoned and his report suppressed. It was only after eight years that the admiral was allowed to go home to die, at the request of Napoleon.

All of this is presented by Robert J. King with many maps and drawings, and with vast margins, in what must be one of the most beautiful productions of a historical work in recent years. But proof of any connection between Malaspina's theories, or Matra's, and the decision makers is not here. And, so far, not anywhere else either.

STEPHEN B. BAXTER
Chapel Hill, North Carolina

UNITED STATES

CHRISTOPHER LASCH. *The True and Only Heaven: Progress and Its Critics*. New York: W. W. Norton. 1991. Pp. 591. \$25.00.

Christopher Lasch has written a very personal book, at once passionate and wonderfully rich in ideas, formless and powerfully argued. Based on a remarkable command of modern European and American intellectual history, it is a work of history and political philosophy. As political theory, the book offers a provocative and important consideration of the "desiccation" of American culture and politics. But it is also, at least in parts, a major work of intellectual history.

In several instances—his discussion of the current romance with civic republicanism in American historiography, and his accounts of Thomas Carlyle, Ralph Waldo Emerson, Orestes Brownson, Georges Sorel, Herbert Croly, and Reinhold Niebuhr—Lasch challenges conventional readings in significant ways. His critique of the republicanism theme is especially timely and important. His targets are neither J. G. A. Pocock nor Gordon Wood, but rather those who have drawn on their scholarship with the aim of recovering (inventing?) a more democratic and morally satisfying alternative to liberalism. All of this involves caricatures of both republicanism and liberalism (usually the adoption of C. B. Macpherson's "possessive individualism" as the core meaning, an interpretation discredited twenty years ago by John Dunn). Lasch shows that nineteenth-century critics of industrial capitalism drew on liberalism, republicanism, and protestantism (often in combination) depending on circumstances.

He delineates the twentieth-century American liberal tradition in a way that reveals its excessive faith (with only a few exceptions) in the powers of reason and the logic of progress. He presents the Keynesian revolution as a version of Gompersism: not perfection but the promise of unending improvement. What liberals have done, he argues, is to substitute the promise of consumption for the politics of participation. The priority of the distribution question has closed the American political imagination to the possibility of smaller units of economic and political life that would invite greater participation and responsibility.

Although he praises the petite bourgeoisie's sense of limits (and does so limitlessly), he recognizes that more is needed. Hope rather than progress offers inspiration for Lasch. Unlike progress, hope can keep close company with a sense of the tragic, which he finds in Emerson as well as Niebuhr.

Lasch is also anxious to tell us how to be intellectuals in a democracy. His critique of the contempt that liberal intellectuals have had for the parochial and limited worlds of most Americans is well targeted and passionate. Among the works he addresses are Walter Lippmann's studies of public opinion, Robert Lynd's

Knowledge for What? (1939), Gunnar Myrdal's *An American Dilemma* (1944), T. W. Adorno's *The Authoritarian Personality* (1964), Robert Lane's *Political Ideology* (1962), and, somewhat more obliquely but nevertheless tellingly, Richard Hofstadter's *Age of Reform* (1955) and *The Paranoid Style in American Politics, and Other Essays* (1965). Against such "dissociated" criticism, to use a distinction developed by Michael Walzer, Lasch proposes a "connected" criticism that shares something with the way of life being discussed. At present he sees a destructive culture war based on class: the educated and secular upper middle class against the less well-educated and very vulnerable lower middle class. The harvest of that war has been the right-wing populism of the Reagan years.

In such a sprawling book it is difficult to isolate a moral center. But were I pressed to choose one, it would be his consideration of Martin Luther King, Jr. Lasch offers a sympathetic account of King; he clearly considers him an authentic American hero. Yet he finds a disturbing pattern in King's movement from being a particularistic leader (southern, black, religious) to a social democrat speaking a universalistic language of "humanity," a shift that coincides with King's movement from the South to northern cities. Partly, Lasch is following Hannah Arendt in worrying about a politics that is reduced to welfare administration. But he is also anxious to defend particularism, even parochialism. And here I resist his argument. I acknowledge without hesitation the case for greater respect for and engagement with parochial points of view. But do the undeniable problems with elite universalism (including its denial of the universality of parochialism) justify so complete an embrace of the parochial, which can so easily become narrow and bigoted? He confines himself to one extreme of what ought to be a mutually enriching play between the historical construction of the particular and the general, between parochial homogeneity and cosmopolitan diversity. He fails to see in cosmopolitanism, a word he seems to hold in contempt, a possible means of reconceptualizing this terrible choice, as Randolph Bourne recognized in the era of World War I. It is a missed opportunity of some consequence, for without something larger than the provincial, and more concrete than Josiah Royce's loyalty to loyalty, which he commends, there is no ground, I fear, on which to establish the recognition and justice for which Lasch cares so deeply.

THOMAS BENDER
New York University

JAMES HOOPES. *Consciousness in New England: From Puritanism and Ideas to Psychoanalysis and Semiotic*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1989. Pp. viii, 294. \$36.00.

The subtitle of this provocative new book gives only the barest indication of what James Hoopes means by "consciousness" in New England. His is not a history of mentalities, climates of opinion, or even that old intellectual chestnut, the New England Mind. Rather, what Hoopes offers is a 250-year history of, in his words, the "consciousness concept," a concept informed by what New England theologians, philosophers, and psychologists thought about the problems of religious doubt and mental anguish, and the corresponding solutions of conversion and cure.

By the "consciousness concept" Hoopes means the idea or cluster of ideas that confidently asserted the availability of the mind's contents to direct an unmediated examination by the self. Before the arrival of Lockean and Cartesian philosophy, Hoopes argues, most Puritan writers had treated the meaning of their thoughts and feelings as accessible to conscience only through the protracted and agonizing rigors of self-examination and collective interpretation. Once thinkers such as Jonathan Edwards began to make their accommodations to the consciousness concept, however, the way (if not the heart) was prepared to pare down the long, ruminative process of conversion to the self's immediate judgment on its own experience. Free will in the "external" world required free access to the "internal" world. To be born again under these auspices, consequently, was to be spared the interpretive labor pains that had marked off the Calvinist conversion as the singular life event it had once been. Indeed, it was just the wish to avoid such birth trauma, Hoopes seems to suggest, that led free-will evangelicals to seize the promise of immediate introspection as their own immaculate conception.

Yet where contemporaries (not to mention twentieth-century historians) might have regarded this conviction in the transparency of subjective life as no more than the evangelical subordination of head to heart, Hoopes treats it instead as an implicit and therefore unwitting intellectualization of subjective life. Following Norman Fiering, he suggests that this alienation of subjective life as an object of perception rather than interrogation laid Puritanism open to the kind of spiritual technology that would produce both the sentimentalist subversion of traditional piety and the eventual triumph of modern psychological theory.

This book, however, is anything but a reprise of the critical histories of the rise of therapeutic culture by Philip Reiff, Christopher Lasch, and T. J. Jackson Lears. To the contrary, it is from start to finish a history of thought about thought, a work in which the main characters struggle—with partial success—to contain the imperial claims of the consciousness concept they have otherwise honored. After two preliminary chapters on conversion and consciousness, Hoopes walks the reader through the dense thickets of the philosophy of mind in New England, extending from Edwards through Samuel Hopkins, Charles Finney, Horace Bushnell, Oliver Wendell Holmes,

G. Stanley Hall, Ebenezer Gay, and Orestes Brownson, to Charles Peirce, William James, and John Dewey, and finally to Morton Prince, Josiah Royce, and James Jackson Putnam. It is a long and often exhausting journey, but one in which the tireless Hoopes never fails to show how seemingly obscure and unfamiliar branches of nineteenth-century religious and philosophical disputation all spring out of the same conceptual trunk, as if each of his key figures were seeking the kind of metaphysical nourishment—be it simply the air of mystery—denied to the modern soul by the spurious certainties of direct experience and the consciousness concept.

Along the way, Hoopes treats his audience to a number of interesting rereadings of major American thinkers, especially Edwards, Peirce, and James. But most intriguingly, he manages to end his odyssey close to where he began, by arguing that the late-nineteenth-century interest in the "unconscious" restored at least some of the subjective uncertainties that had made the interpretive requirements of conversion such an important element of Calvinism. Acknowledging the startling impact of psychoanalysis on Victorian moralism, Hoopes nonetheless argues that the discovery of the unconscious was a fundamentally conservative response to the overweening ambitions of the consciousness concept: establishing a place in the heart to be interpreted rather than introspected.

Although Hoopes concedes that this psychological reconstruction of the conversion protocol fell far short of a complete victory for interpretation over intuition, he does present a story of change: of the rise and partial decline of the consciousness concept—the Cartesian Moment, if you will—in New England. In so doing he challenges the narratives of obsessional repetition that historians such as Sacvan Bercovitch and John Owen King have laid over the same historical materials. In a short afterword Hoopes argues eloquently against their practice of textual or discursive history as an inherently or structurally conservative enterprise, indifferent to change except as change is understood as the inevitable failure to copy the past exactly. Borrowing from Charles Sanders Peirce, who is at once an object of and aid to his reflection, Hoopes insists that neither symbols nor their interpretations are ever wholly arbitrary, that thought is itself an action that can affect itself, and that the record of those effects is itself worthy of interpretation. In this way, the book presents itself as a contribution to the historical (and historiographical) debate it reconstructs.

For all its affirmations of the indispensable and independent activity of thought, there is a strangely static and hermetic quality to Hoopes's argument that unexpectedly parallels the insularity of the discursive or textualist approaches to which it otherwise stands opposed. For when all is said and done, most of Hoopes's thinkers seem to spend most of their energies pushing, struggling, fending off, and eventually

capitulating to the "overwhelming . . . weight of the consciousness concept" (p. 96). In a book devoted to thought's impact on thought, there is no other kind of weight to put on the scale: not society, not culture, not even biography. This is a shame, for Hoopes's work is a gem: hard and luminous. Only the setting disappoints.

JEAN-CHRISTOPHE AGNEW
Yale University

STEPHEN FOSTER. *The Long Argument: English Puritanism and the Shaping of New England Culture, 1570–1700*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Virginia. 1991. Pp. xx, 395. \$39.95.

This learned book by Stephen Foster presents Puritanism as a "loose and incomplete alliance of progressive Protestants, lay and clerical, aristocratic and humble" (p. 5) held together by a common antagonism to the "paper Protestantism" (p. 38) of Elizabethan and Jacobean England. Despite their disorganized changeability—Foster's Puritans are a far cry from Michael Walzer's revolutionary cadre (*The Revolution of the Saints* [1965])—they were the vanguard of "a gigantic national experiment . . . to raise the state of civility and Christianity of the English people beyond the merely nominal" (p. 6). The "central duality" of this "inherently paradoxical" movement (p. 106) was its effort to make two urgent commitments work together. It was devoted, on the one hand, to the purity of ordinances, regular gospel preaching, and the defense of small voluntary religious communities whose existence constituted a rebuke to the national religious establishment; on the other hand, it sought to compel the Church of England to provide "the nation with moral direction . . . as the custodian of its high culture" (p. 3). The American version of this dual experiment was continuous with the English—not so much as a break-away radical faction, but as a group grown exhausted from the effort of holding its two halves together. A "duality between the insular and the comprehensive . . . had always been at the heart of the movement" (p. 27) and continued both to animate and vitiate Puritanism in the New World, as, for example, in "the elaborate ceremonies of admission" that "were held to have a didactic and evangelical effect on the onlookers" (p. 173).

This theme of duality is, in Foster's account, "recurrent, even obsessive . . . from the admonition of 1572 to the heyday of Increase and Cotton Mather" (p. 34), and, indeed, through the Great Awakening, whose evangelical "message [New Englanders] had been born and bred to hear" (p. 296). His Puritans always "straddled the gap between establishment and sect" (p. 9), and as their movement took shape, they

made "a discovery" that, "quite apart from polity, the culture of the age offered a multitude of means to draw people voluntarily into a disciplined life and purposive society" (p. 64). There are many such tense phrases in Foster's book that contribute to a picture of Puritanism as a kind of uneasy political convention—not unlike the Long Parliament itself—with platforms, keynote addresses, reports from subcommittees, and so forth. What is missing from Foster's account is a vivid sense of why these people, laity and clerics and politicians, were driven to give their lives to what he himself calls "insoluble logomachies" (p. 68), although he does seem to find at least some of the inner energy of Puritanism in the doctrine of predestination, which "alone gave each and all their own fate, one they were charged to work out for themselves, in fear and trembling to be sure but also in discovery and purposive struggle" (p. 289).

Foster has written a kind of reference book in narrative form, full of excellent short discussions of vexed issues in Puritan history: the difference between seventeenth-century and modern conceptions of hypocrisy; the "severely guarded optimism" that lurked within the Puritan commitment to disseminating literacy; the distinction between the first immigrants to New England and the second wave of the later 1630s, who came with a keener sense of defeat and a taste of Laud's fury. All these arguments, and many more, make this a significant contribution to the field in the lineage established by Patrick Collinson (*The Elizabethan Puritan Movement* [1967]), whose version of Puritanism Foster modifies, refines, and extends.

ANDREW DELBANCO
Columbia University

GUNTHER BARTH. *Fleeting Moments: Nature and Culture in American History*. New York: Oxford University Press. 1990. Pp. xxii, 222. \$29.95.

Gunther Barth's work is a compact, thoughtful essay on the relationship between humans and the natural environment in North America from the age of exploration to the twentieth century. A meditation, enlivened by fascinating anecdotes and peppered with insights, the book does not present a comprehensive study of its subject but rather offers a gloss on the three centuries—and more—of writing that has sought to define the special qualities of "nature's nation." Starting from the premise that culture and nature delineate different spheres (the "mental and physical constructs that deal with [the] environment," and "those areas least touched by them" [p. 8]), Barth concentrates on three situations in which humans have operated at the tenuous meeting-place between nature and culture: the search for a "wilderness passage" that would connect the Atlantic and the Pacific trade routes, the Lewis and Clark expedition

of 1804–06, and the interconnected movements to develop garden cemeteries and urban parks in the latter part of the nineteenth century.

In each of the three substantive chapters, Barth offers memorable insights. In his discussion of the early contacts between Europeans and Native Americans, the author discovers moments when ethnocentricity yielded to genuine admiration, when cultural contact did take place, from the assimilationist *coureurs des bois* to the utopian theorist Christian Priber and others who lived among the Indians and adopted their ways. His chapter on the Lewis and Clark expedition presents a fascinating reading of the explorers' journals as a battleground between cultural constructs and immediate observation; his discussion of Merriwether Lewis's growing identification with the grizzly bear is particularly notable (pp. 96–101). The chapter on garden cemeteries and urban parks makes an important connection between these two movements, and in his detailed discussion of the creation of Golden Gate Park he argues for the brief union of the forces he calls "demopiety," or faith in ordinary people, and "geopiety," or faith in God evoked by landscape (pp. 150–51).

This thoughtful and interesting book is, however, circumscribed by its own assumptions that nature and culture are completely separate, the former including the native inhabitants of the land, the latter the emerging Euro-American civilization. It does not seriously credit the idea that nature is always, for humans, perceived through culture. Furthermore, although it has moved a long way from the "march of progress" narrative of American history, its subjects are still the hardy pioneers and explorers, the public officials and city planners who have traditionally been seen as "the Americans" interacting with "the wilderness." The author does not seem interested in recent scholarship on women and children, immigrants and African Americans, and on the frontier. In fact, Barth can be seen as the latest in a line of descendants of Frederick Jackson Turner, whose lecture on the significance of the frontier in American history will soon see its hundredth anniversary. Like Turner, Barth formulates a vision that could be considered nostalgic, celebrating a past in which American wilderness was yet unravished by greed and exploitation. But in the 1990s it is no longer possible to view the frontier experience uncritically. The "fleeting moments" of the title are fragile, transitory scenes from our past, not a past of towering frontier heroes leaping into birch-bark canoes but of fallible humans striving for moments of concord that our twentieth-century vision knows to be unstable. Barth revisits the American frontier wondering, with Robert Frost, what to make of a diminished thing.

JOY S. KASSON
University of North Carolina,
Chapel Hill

CARL N. DEGLER. *In Search of Human Nature: The Decline and Revival of Darwinism in American Social Thought*. New York: Oxford University Press. 1991. Pp. x, 400. \$24.95.

Carl N. Degler has written an unusual book, one that uses historical analysis to make a brief for a particular social science theory. A partisan of sociobiology, he examines the decline and return of Darwinism in American social thought, hoping to convince historians and social scientists to abandon their long dismissal of biological explanations of human behavior. Accomplishing this aim while meeting the standards of historicism is a difficult task. Unfortunately, he does not succeed. He falls into the error of the social scientists themselves, by failing to ground his own theory in history.

The historical question Degler asks and the answer he gives are both strikingly asymmetrical. He asks why social scientists turned away from biological to cultural explanations of social behavior and then why biology returned. He does not begin at the beginning and ask why social scientists adopted biological explanations in the first place. Charles Darwin's scientific proof of the "continuity" (p. 8) of animals and humans is presented as self-evident ground for the social application of Darwinian biology. Degler nevertheless shows that these applications produced, besides some insight into the importance of heredity, invidious theories of race, sex, intelligence, and eugenics.

Degler ascribes the decline of Darwinian social theory to the "ideology" of the social scientists—their "commitment to the ideological principle of equality of opportunity" (p. 189). Fueled by the work of Franz Boas, changing attitudes toward race and ethnicity in the 1920s and New Deal era, and reaction against Nazi racism, this egalitarian ideology moved to erase not only the older invidious theories but also virtually all trace of biological causation in the social scientific study of social behavior. Science and innovative scholarship were factors in this massive shift, but played only a limited role (p. viii).

When Degler turns to the rise of sociobiology, however, he shifts his ground in a remarkable way. Here his explanation is wholly different. The scientific inadequacy of cultural and environmental theories and "new and exciting answers and theories in the biological sciences . . . converged in the 1950s and 1960s" to revive interest in the subject (p. 232). He grants a minor role to social scientists' attraction to the scientific certainty biological science seemed to offer. But he denies outright that a "conservative political climate" had any effect, because "the political atmosphere can hardly be described as conservative" during the 1950s and 1960s (pp. 226–27). While Degler devotes a chapter to examining the rise of egalitarian ideology in the 1920s and 1930s, this single statement constitutes his entire examination of the political climate of the postwar decades and of the

1970s, when sociobiology took substantial form. There is no mention of the postwar hope that American norms were enshrined in universal nature (an idealistic moment from which Degler's biological enthusiasm apparently derives); no mention of the increasingly conservative Cold War liberalism that dominated the universities well into the 1960s, bringing with it a new emphasis on the limits of social action; no mention of the countermovement toward the Left that polarized national and campus politics and led some social scientists to search for a new and still harder science to replace the pluralistic behaviorism discredited by the Left; no mention of the rise of the Right in national politics in the 1970s and the new sources of funding that opened for conservative scholarship.

Degler has another reason for discounting politics: "many" proponents of biology are "liberal" (p. 227). He is able to list in that category, however, only a few of the many sociobiologists he quotes (p. 319) and gives no evidence of the political thinking of any sociobiologist. Indeed, the description of political orientation throughout the book is decidedly thin. Part of the problem is with Degler's categories. "Liberal" is never defined. The chief categories of analysis are "conservative," defined as defense of the status quo, and "reformist," defined as favoring change to improve the world (pp. 13, 42, 112). With these definitions, virtually all the social scientists at the turn of the century are termed reformist (p. 13); eugenics is termed reformist (p. 42) and not reformist (p. 145); and sociobiologists are freed from "conservative bias" because "many of the early twentieth-century social scientists were reformers rather than conservatives, yet many of them looked to biology in some fashion" (p. 319). This analytical net cannot begin to catch the political orientations of American social scientists nor the changing relations between politics and social scientific work.

Degler argues that there is no necessary connection between sociobiological explanations and political stance. He should take the argument further: there is no necessary connection between most of the premises of social science and the conclusions that are drawn from them. That is one reason why ideology is so important a factor in social science, and especially so in an area as fraught with gaping ignorance, analogical speculation, and high-stakes political consequences as this one. He realizes that this situation obtains early in the century as between racial and cultural explanations of group behavior, but not that it still obtains in regard to sociobiology.

There are some good things in the book. The discussion of changing race relations in the North as a basis for shifting attitudes toward race is excellent. The five chapters that survey the rise of cultural and environmentalist thinking in the social sciences are very useful, despite some errors. Degler traces the influence of Boas and shows the sharp differences in viewpoint that often prevailed between social science

disciplines and the different logic often applied to issues of race, ethnicity, class, and gender. In the last five chapters of the book, he presents interesting, but selective, discussions of sociobiology to show what it can do. Here I disagree with his judgment that sociobiology is concerned almost entirely with what is universal rather than differential in human behavior and that sociobiologists are now fully cognizant of the role of culture. To a critical eye, so many mistakes of the old sociobiology reappear in the new that much of the enterprise remains suspect.

DOROTHY ROSS
Johns Hopkins University

RICHARD R. JOHNSON. *John Nelson, Merchant Adventurer: A Life between Empires*. New York: Oxford University Press. 1991. Pp. ix, 194. \$29.95.

This charming, brief biography by Richard R. Johnson takes the reader to the forest and sea frontier between New England and New France in the years 1670–91. John Nelson, a teenager when he arrived in Boston in the late 1660s, was nephew, heir, and protégé of his uncle Sir Thomas Temple, an early proprietor of Nova Scotia. With the assistance of his uncle's partners, Nelson at once began to trade between Boston and French-held Acadia. Port Royal on the west coast of Acadia was the largest community; on the Maine coast Baron Saint-Castin's home at the mouth of the Penobscot River was the most important, but there were many other points of trade. Nelson was welcome in them all.

Men such as Nelson brought European manufactures and carried away furs, deer skins, and fish. In addition there was smuggling in which illegal imports were landed by the shipload on the Maine coast and taken piecemeal down to Massachusetts in small boats.

Nelson quickly established himself with the Boston merchants, but he kept aloof from Puritan New England. He became a member of the Ancient and Honorable Artillery Company, but not a Freeman of Massachusetts. Nor did he ally himself with Sir Edmund Andros when he came as Governor-General of the Dominion of New England. When Andros's government fell in 1689, Nelson commanded the militia that forced his surrender. Johnson portrays Nelson as the consummate young gentleman: polished, adventurous, capable, and independent.

The imperial contest for control of Acadia began in earnest in 1688 with the outbreak of King William's War. Nelson was captured in 1691 by a French man-of-war off the mouth of the St. John River and sent prisoner first to Quebec (up the St. John by canoe, then overland to the St. Lawrence, fifteen days), and finally to France. A prisoner for six years, he at last extricated himself and reached London. There he tried his best to advise the English government how to conquer Acadia. His plans were not

immediately taken up, and Nelson returned to Boston in 1698. Almost at once he retired to his farm outside Boston.

As romantic and engaging a figure as Nelson was, he was ineffective as an empire builder. In 1690 he was the logical man to lead the attack against New France, but Sir William Phips was chosen instead. In England in 1696, fresh from France with inside information, Nelson could not get his strategies adopted. These were perhaps the penalties of a lifelong sense of superiority to ordinary allegiances.

The biography is a model of research in English, Canadian, and American archives. Johnson sheds bright new light onto a corner of the history of empires usually in deep shadows, and he does so with grace and judgment.

MICHAEL G. HALL
University of Texas,
Austin

JAMES TITUS. *The Old Dominion at War: Society, Politics, and Warfare in Late Colonial Virginia*. (American Military History.) Columbia: University of South Carolina Press. 1991. Pp. xii, 213. \$29.95.

James Titus has written an important book in colonial American military history that focuses on how the province of Virginia fought the Seven Years' War. Concentrating on the impact of this conflict on the colony's society and politics, Titus places "Virginia's military experience in its wider social and political setting" and shows how the conduct of Virginians in this war helps to explain much about the nature of the province's society in the mid-1750s. He notes that although Virginia's military policy changed over time, nevertheless more than half the war was fought with a European-style army "drawn largely from the mud-sill of provincial society" (p. ix). Although this policy seemed sound in 1754, the colony learned that the lower social orders refused to go along with the scheme. Thus, midway through the war governmental leaders were compelled to abandon attempts to coerce conscripts to do military duty and replaced them with volunteers who agreed to serve for bounties. Essentially, then, the colonial elites were required to allow soldiers to set their own terms for enlistment. This raises serious questions about whether historians have been correct in interpreting Virginia provincial society as politically and socially deferential. While Titus concludes that this interpretation is not wrong, he nevertheless also concludes, on the basis of his study of the lower ranks' refusal to be complaisant cannon fodder, that the common people drew a line at acquiescing to "a policy that might result in [their] taking the field for an extended period of soldiering" (p. 36). The lower classes were supported in this viewpoint by some elite Virginians who bowed to public opinion against an unpopular war being fought a long distance away on the frontiers. Other

gentlemen only caved in to the recalcitrance of the "lesser sort" because government in mid-eighteenth-century Virginia had not the coercive power to force citizens to do what they refused to do of their own free will.

According to Titus, the reasons for the lower orders' reluctance to be pushed about during the war are varied. Partly it was a matter of their individual calculations of danger and suffering. But more importantly their resistance was grounded in their acquaintance with slavery. Poor whites valued freedom no less than the upper and middle classes, and they did not wish to be degraded as blacks were in involuntary servitude. Also, despite the aristocracy's belief that resisters to military service were merely criminals, the lower classes saw their actions as political in that they were using the only means at hand to ward off threats to their personal liberty. In finding the common people of Virginia glad to serve once they had been paid bonuses and a "contract" had been struck, Titus discovers the same phenomenon that Fred Anderson found among Massachusetts soldiers in the same war (*A People's Army* [1984]).

PAUL DAVID NELSON
Berea College

CATHY D. MATSON and PETER S. ONUF. *A Union of Interests: Political and Economic Thought in Revolutionary America*. (American Political Thought.) Lawrence: University Press of Kansas. 1990. Pp. x, 237. \$25.00.

Historians have long speculated on the motives of the authors of the federal constitution. The literature is rich on individual participation and social, political, and economic conditions that contributed to the formation of a new government in 1787. Significant scholarship on republicanism in recent years has prompted historians to reexamine how the delegates to the Philadelphia convention were able to reshape their idealism into a system that could deal with the economic crisis at hand. Cathy D. Matson and Peter S. Onuf successfully reconstruct the debate over American federalism as it related to the role of government in the regulation of the economy. Specifically, they emphasize "how the rapidly changing context of American federalism shaped the development of this thinking (political and economic thought) and how, in turn, changing ideas contributed to the reconception and reconstruction of the Union" (p. ix).

The authors explore the development of political and economic thought in the American colonies and how abstraction became reality when the crisis with Great Britain became imminent. There were two time-honored ideals that most Americans wished to achieve. One was the creation of a classic republic based on virtue and self-restraint; the other was private enterprise that guaranteed international free trade. The delegates arrived in Philadelphia in 1787 because the first effort to provide the concept of

republicanism and free trade, the Articles of Confederation, had failed. Matson and Onuf are most successful in their effort to demonstrate how the nationalists who gathered during that fateful summer became federalists in order to create an acceptable constitutional design. They conclude that the "great achievement of the Philadelphia Convention was to establish a framework for national politics that accommodated powerful state-particularists and sectional tendencies" (p. 123).

Implementation of the new federal constitution is the subject of the last two chapters of the volume. The authors argue for a more balanced assessment of the Antifederalist contribution to the ratification debate; they do admit, however, that the Federalists were able to use the economic crisis in order to sell their concept of a more perfect union. Moreover, James Madison and Alexander Hamilton, among others, were able to conceptualize the interdependence of economic growth and the stronger central government. This book is based on extensive use of primary sources and secondary works related to republicanism, federalism, and free trade. The reader is not required to accept the thesis of the volume in order to appreciate the authors' valuable contribution to the further understanding of the creation of the new federal government.

ROBERT K. RATZLAFF
Pittsburg State University

JOHN K. ALEXANDER. *The Selling of the Constitutional Convention: A History of News Coverage*. Madison, Wis.: Madison House. 1990. Pp. ix, 246.

John K. Alexander has read every surviving newspaper and magazine from the call for a Constitutional convention in the winter of 1787 to the final printing of the Constitution in these media by the fall of that year. Like less-systematic observers, he finds a propaganda campaign designed to support the work of the convention. If we follow Alexander, we will give less weight to republican ideology or economic analysis in the public discourse over the federal government, for he finds little of this in the press. He invites us to see press comment as "news management" (p. 8). This term, and another, "media hype" (p. 177), may seem overblown for isolated weekly papers, none reaching more than a few thousand subscribers. But Alexander does not claim more power for the press than was recognized by observers in his period. David Humphreys saw editors striving "to prepare the minds of the Citizens for the favorable reception of whatever might be the result" of the convention (p. 9).

The signal contribution of this book is the clarification of how political argument spread. By counting reprints, Alexander enables us to see what news was available, when, and where. For example, forty American newspapers elaborated on Rhode Island's

"quintessence of villainy" (p. 24) in resisting the convention call, but there were never more than ten papers urging caution about what might happen in Philadelphia. This book is a fascinating guide to how the miniclimates of American politics in the 1780s lived with, and without, information.

There are problems, however, in construing this pattern as salesmanship. "Selling" and "management" are problematic terms because they seem to accommodate any evidence that comes to hand. When the delegates took their seats in Philadelphia, only two newspapers printed the calls of the "West-Chester Farmer" for sharply reducing the power of states and creating a federal government with a president and two-house legislature. Alexander argues that by hushing up a radical idea, printers built support among a cautious public. But had forty newspapers published the "West-Chester Farmer," one could just as plausibly argue that the press was preparing the minds of the people for the Constitution. Similarly, Alexander finds that "proconvention editors . . . apparently refused to reprint anything that suggested the delegates contemplated establishing a monarchy" (p. 130). Had they published these Antifederalist arguments in order to refute them, Alexander's point would be the same. It is a leap of faith to imagine that every modulation of political argument reveals a grand design. Inattention and inertia are equally plausible explanations for what eighteenth-century editors put in their newspapers.

Before "news management" can be seen, we need a grounding in the occupational ethos of printers and their trade strategies in other political campaigns. The printed word itself took on new functions in the making of the republic, and Alexander might profitably have examined the social context of reading as explored by Richard D. Brown, William J. Gilmore, and Michael D. Warner. Alexander's careful and fair-minded book has not given the full dimension of political advocacy among news readers.

THOMAS C. LEONARD
University of California,
Berkeley

GARY B. NASH and JEAN R. SODERLUND. *Freedom by Degrees: Emancipation in Pennsylvania and Its Aftermath*. New York: Oxford University Press. 1991. Pp. xvi, 249. \$24.95.

This exploration of the emancipation process in the first state to pass an abolition act (in 1780) is the collaborative effort of two historians who have in previous excellent books stressed the narrow limits of abolitionism and the harrowing restrictions for freed blacks in eighteenth-century Pennsylvania. Jean R. Soderlund in her *Quakers and Slavery: A Divided Spirit* (1985) demonstrated that the Quaker protest against slave keeping was long and fiercely contested by many Pennsylvania and New Jersey Friends, who held onto

their slaves long after the yearly Meeting proscribed slave trading in 1758. And Gary B. Nash in his *Forging Freedom: The Formation of Philadelphia's Black Community, 1720–1840* (1988) focused on the desperate struggles of free blacks to find work, build a community, and sustain their own culture within an increasingly hostile, racist environment. In this present work, the two authors restate a good many of their previous positions, but they build on their earlier investigations to present a broader—and even gloomier—overall argument.

The abolition of slavery in Pennsylvania, according to Nash and Soderlund, was half-hearted at best. Although the slave population in this state was tiny—less than 3 percent in 1750 and barely 2 percent in 1780—the slaveholders were exceedingly reluctant to give up their human property. The authors tabulate 1,080 individual manumission cases from Philadelphia and Chester counties between 1698 and 1800 and find as expected that Quakers were far readier than Anglican, Presbyterian, and German church members to free their slaves. They also find that only 259 slaves in this sample were manumitted before 1775, as against 609 between 1775 and 1790. Nash and Soderlund argue that the slaves themselves contributed greatly to the decline of the institution during the revolution by running away in large numbers. And they stress that the Gradual Abolition Act passed by the Pennsylvania Assembly in 1780 actually freed no slaves but rather provided that children born into slavery after 1780 would be freed at age 28—after giving their masters long years of prime labor. It appears that many young slaves were manumitted after 1780 on condition that they bind themselves into indentured servitude until age 28; between 1780 and 1820 nearly 3,000 free blacks were indentured on these terms in Philadelphia, in effect sustaining a semislave status. While acknowledging that the members of the Pennsylvania Abolition Society made valiant efforts to protect and assist the freed blacks, Nash and Soderlund conclude that most white Pennsylvanians were chiefly interested in reincorporating the freed blacks as cheap, dependent laborers.

The argument of this book is more persuasive than the tone, which is persistently judgmental and censorious. The authors tend to hold eighteenth-century white Pennsylvanians to late-twentieth-century civil rights standards, and find their performance woefully inadequate. It would seem more appropriate to set this emancipation story within the continuum of revolutionary-era racial attitudes and actions. One also misses the richly detailed account of human agency and social interaction that makes Nash's *Forging Freedom* such a compelling work. In this book the framework is statistical, and individual actors seem less significant. These reservations aside, Nash and Soderlund are to be congratulated for completing a very impressive trilogy of books that depict in an

insightful fashion the character of abolitionism, emancipation, and black life in early Pennsylvania.

RICHARD S. DUNN

University of Pennsylvania

CHRISTOPHER MCKEE. *A Gentlemanly and Honorable Profession: The Creation of the U.S. Naval Officer Corps, 1794–1815*. Annapolis, Md.: Naval Institute Press. 1991. Pp. xv, 600. \$46.95.

Although there has been considerable research on the operations of the American navy in the early national period, little has been written on how the navy actually functioned. Nor has anyone explained how the navy developed so quickly into a force capable of successfully prosecuting three wars between 1798 and 1815. In this study, based on twenty years of research in diverse sources, Christopher McKee seeks to understand the navy by examining the development of the officer corps.

McKee focuses on those officers who were on the promotion ladder—midshipmen, lieutenants, masters commandant, and captains—but he also has much to say about the others—the sailing master (who navigated the ship), the purser (who oversaw its finances and supplies), the chaplain (who was the midshipmen's teacher as well as the captain's administrative assistant), the captain's clerk, the surgeon, and the surgeon's mate. Hierarchy and deference were "the bedrock principles of the naval world" (p. 28), and yet it was preeminently a young man's service. Only one man in ten was promoted beyond his initial rank, and by the time they had reached the age of thirty most officers (as well as enlisted men) had forsaken the rigors of cruising for less taxing pursuits.

According to McKee, Virginia, Maryland, and the District of Columbia provided a disproportionate share of the navy's midshipmen. Politics played a part in securing the coveted midshipmen's warrants, but promotion was by merit, or at least the senior officers' perception of merit. Naval officers frequently took furloughs to work on merchant vessels, some to acquire the sort of hands-on experience that was forbidden to gentlemen on warships, others to supplement their income.

McKee provides an excellent analysis of the duties of the officers, navy finances, death and resignations, alcohol abuse (predictably widespread), and homosexuality (surprisingly rare). He also has a fascinating section on crime and punishment. Enlisted men who were punished for infractions of the rules were usually clamped into irons and/or whipped. Twelve lashes was common, but the number could run as high as 320. Contrary to popular belief, McKee demonstrates that even several hundred lashes was unlikely to be fatal.

In showing how the Navy Office worked, McKee stresses that much of its business was conducted

without leaving a paper trail. He provides good thumbnail sketches of many naval officers as well as the personnel in the Navy Office. McKee calls Samuel Smith, the bruising wheelhorse of Baltimore politics who served as acting secretary of the navy in 1801, "a wise and reflective man" who shaped the officer corps when he oversaw its reduction after the Quasi-War (p. 416).

One might quarrel with some of McKee's techniques—his failure to provide more comparisons between officers and enlisted men and his penchant for seeing personality traits in the portraits of his subjects. But overall his work, which goes far beyond his modest title, is a splendid achievement, one that ought to inspire and inform anyone interested in the development of the American navy.

DONALD R. HICKEY

*U.S. Army Command and General Staff College
Fort Leavenworth, Kansas*

DONALD R. HICKEY. *The War of 1812: A Forgotten Conflict*. Urbana: University of Illinois Press. 1989. Pp. xiii, 457. \$32.50.

There is much to applaud and little to fault in Donald Hickey's clearly and compellingly written book. Initially I experienced misgivings about having to fight my way through the war from Detroit to New Orleans one more time, but I quickly became engrossed in Hickey's narrative. Balance is excellent and all aspects of the war are fairly treated: military, naval, diplomatic, political, financial, and social. Particularly notable is Hickey's excellent narrative and analysis of the Baltimore riots of 1812. The author is evenhanded in his judgments, and he has made especially effective use of newspaper sources. If the book has a weakness, it is a bias in favor of U.S. documentation; little is drawn from manuscripts located in British archives.

Any review in the *AHR* must necessarily be concise; students of the War of 1812 should not miss Reginald Horsman's extended review of Hickey's work (*Diplomatic History* 15 [1991], 115–24), in which Horsman compares in detail the present book with the full spectrum of recent work.

With Hickey's contribution added to earlier works by Horsman (*The War of 1812* [1969]), John K. Mahon (*The War of 1812* [1972]), and J. C. A. Stagg (*Mr. Madison's War* [1983]) there now exist four relatively recent one-volume histories of the War of 1812. In my opinion, the need for further general narrative histories of the origins and course of the war is at least questionable. One could spend all one's time listening to the same work by Beethoven as played by different orchestras and conductors, but it might be intellectually fruitful to expand one's musical horizon to include some Igor Stravinsky or some John Adams. There is much still to be said about the War of 1812, but what are needed are fresh ap-

proaches and novel methodologies. Military and naval history of the war told from the perspective of wellness, morbidity, and mortality would be a fascinating study for which there are ample sources. The Public Record Office in London bulges with unexplored British records of the conflict. A detailed analysis of the naval and combined operations on the Great Lakes is sorely lacking. One hopes that it is in directions such as these that would-be historians of the War of 1812 will now turn their attention.

CHRISTOPHER MCKEE
Grinnell College

JEAN V. MATTHEWS. *Toward a New Society: American Thought and Culture, 1800–1830*. (Twayne's American Thought and Culture Series.) Boston: Twayne. 1991. Pp. xi, 188. Cloth \$26.95, paper \$12.95.

American cultural and intellectual history seems to have come of age. After shrinking before the assault of social historians over the last quarter century, the study of ideas has reemerged from hiding in recent years chastened yet strengthened. Having jettisoned much of their old baggage of intellectual elitism and disembodied philosophical idealism, its practitioners now seek to analyze American thought in a cultural context while in turn grounding that culture in the materiality of social and economic life. There is no better sign of this revitalization than Twayne's multi-volume "American Thought and Culture Series," under the general editorship of Lewis Perry. Jean V. Matthews's volume offers a distinguished contribution to this project while exemplifying many larger trends in the field.

Examining the period from 1800 to 1830, Matthews offers a skillful synthesis of the most recent research in early nineteenth-century American culture. In part one, she deftly delineates broad tensions evident in the thought of many postrevolutionary citizens: the demise of republican "virtue" and the emergence of ambitious individualism, the decline of the "moderate Enlightenment" and the growing dominance of evangelical Protestantism, the struggle to create a viable national culture in the face of Old World skepticism. In the second section, Matthews explores a variety of cultural endeavors—physical science, belles-lettres, fiction writing, painting, architecture, natural history—and identifies two unifying themes. First, the eighteenth-century study of man as an abstraction gave way to "a greater preoccupation with human differences . . . a concern for individuality, for the uniqueness and distinctiveness of each personality" (p. 71). Second, there appeared a broad instinct for "taking mental as well as physical possession of the expanding natural world" (p. 94), an impulse that produced an imaginative possession and domestication of the same wilderness that was being captured by physical energy and force. The book's final section turns to a new paradigm of American

thought signaled by the election of Andrew Jackson. The intersection of romanticism and cultural nationalism, material progress and popular sovereignty, economic productivity and bourgeois institutions, had gradually produced by 1830 a dramatic democratization of culture. In Matthews's words, American culture now pivoted on "a triumphant assertion of democracy, and liberty was increasingly conceived in individualist terms as a potential for unlimited individual expansion" (p. 151).

Matthews presents the best survey we now have of cultural developments in this period. The text is gracefully written, sensitive to popular as well as elite thought, and cognizant of the subtle assumptions that often link wildly divergent areas of cultural practice. Yet two problems of conceptualization—they may be endemic to this kind of synthesis—linger a bit uncomfortably. First, is "democracy" an accurate way to describe the liberation of individual ambition in a burgeoning market society? Does it truly characterize a polity where women, black slaves, and many white adult males remained marginalized culturally as well as politically? Second, Matthews's schema tends to be descriptive rather than explanatory. The whys and wherefores of the cultural transformation she describes so beautifully—the historical dynamic that produced such changes—remain in the shadows. In other words, the theoretical underpinning of the argument could be made stronger and more explicit.

So while this impressive book provides an excellent overview of a fluid American culture in the early nineteenth century, the reader might be left with the suspicion that Raymond Williams articulated some time ago: the notion of "culture" itself, as a modern construct, must be examined as a compensation for the ravages of everyday life in a competitive marketplace society.

STEVEN WATTS
University of Missouri,
Columbia

ALEXANDER SAXTON. *The Rise and Fall of the White Republic: Class Politics and Mass Culture in Nineteenth-Century America*. (The Haymarket Series.) New York: Verso. 1990. Pp. x, 397. Cloth \$69.95, paper \$22.95.

This study, original in conception and use of historical materials, culminates Alexander Saxton's quarter-century of work on race, class, and culture in the nineteenth-century United States. It definitively establishes the centrality of white racism to American politics and culture by impugning the race-blind, traditional political history that invokes an American dilemma in which ideal commitments to racial equality battle discriminatory practices, and by condemning the new, ethnocultural, political and social history that displaces race by ethnicity.

The thesis in Saxton's historical dialectic is an expansionist, proslavery mercantile and planter elite,

ruling through a politics of deference. Jacksonian mass politics and culture intensified racism as it provided a class antithesis. Although the synthesis, the Republican Party and Free Soilers, began in antislavery, it based the GOP not in the freed blacks but in the white man's, Indian-fighter West. In Saxton's triumph of American capitalism, he implicates race, as seen in conflicts over industrialization placed in the context of southern planter resistance and Indian occupation of western space. Whereas deferential politics constituted a soft, paternalist version of racism, Saxton argues, with race as a variant of class subordination, *herrenvolk* democracy asserted class equality on the basis of racial exclusion. Hardline racism, according to Saxton, moved from the Jacksonian Democrats to the postbellum GOP, setting the context in which organized labor, allying craft and racial narrowness, became exclusionary. The westernizing of national identity nationalized a class identity that withstood the hegemonic crisis of the 1890s in politics and culture. Racism helped defeat the Populist challenge, directly in the South and indirectly through its role in the victory of anti-Populist craft unionism. A newly confident, more inclusive Republican elite softened on class as it used racial Social Darwinism to legitimize post-1896 imperialism. If this triumph complicates Saxton's picture of an elite politics that was hard on class and soft on race, it also renders mysterious the "fall" of his title, unless that word refers to the defeat of Jacksonian versions of a republican, producer ethic by the capitalist, Free Soil, imperialist, Republican synthesis.

Saxton's dramatic narrative is unified by his analyses of literary and political figures, party formations, historical incidents, and new forms of mass entertainment. Together these studies link party, class, and culture in innovative, convincing ways. They include the author's classic interpretation of blackface minstrelsy as Jacksonian ideology, and his equally persuasive placement of the western dime novel in postbellum, free-soil Republicanism.

Saxton prefers not to call himself an American exceptionalist; the exception, in his view, is the class-conscious proletariat of classical Marxism's industrializing Europe. But this historian differentiates the United States from Europe in content to preserve the Marxist method. Ideology and class are the master words of his history, where racism serves class interests rather than being driven by psychological irrationality. U.S. racial politics, to be sure, derived not from some transhistorical psyche but from slavery, westward expansion, the Mexican War, and the use and exclusion of "Oriental" labor. But Saxton's strictures against psychology return to haunt his Marxism. The collective psyche he denies to national identity reappears in class interest. Representative men and women, forbidden in psychological analysis of the nation, are employed in the service of class. The ambiguity in treating individuals as either reflections of shared values or agents of change, a supposedly

disabling feature of psychological history, equally plagues (or enriches) his own accounts. The unsympathetic, positivist historian will also be troubled by Marxism's *a priori*, "theoretical" bedrock, Saxton's term of dismissal for Freud.

Class analysis need not, however, shut out shared cultural fantasies. The author rejects Winthrop Jordan's view, using *Othello* as evidence, that racial phobias in Elizabethan England contributed to enslaving blacks. For the materialist historian, slavery came first. Racial phobias may be in a "dependent relationship to prior, ideological constructions" (p. 89), yet Saxton himself gives historical force to the disgust at Desdemona's desire for Othello shared by such class opposites as John Quincy Adams and George Wilkes. Although Saxton insists that class organizes racism, he equally shows that race is the master principle shaping class. Moreover, although he addresses aversion, he is blind to destructive desire. His pathbreaking treatment of blackface minstrelsy as ideology ignores the paradoxical linkage between hysteria over mixing of bodily fluids and attraction to racial cross-dressing. The denial, displacement, and projection of racial desire is absent from Saxton's white republic. Perhaps that testifies to greater historical grounding than the complementary (yet more literary) volume, Richard Slotkin's *The Fatal Environment* (1985). Or perhaps the historian clings to instrumental rationality, postulates a subject presumed to be in charge, to defend against the horror at the center of his or her story.

MICHAEL ROGIN
University of California,
Berkeley

JOEL W. MARTIN. *Sacred Revolt: The Muskogees' Struggle for a New World*. Boston: Beacon. 1991. Pp. xii, 233. \$24.95.

According to the usual accounts, the Battle of Horsehoe Bend in March 1814 was a victory for Andrew Jackson's frontier army over rebellious Creek warriors. Joel W. Martin would have us understand differently. Analyzing Creek actions from the perspective of the comparative study of religion, he seeks to re-create the lost world of the Muskogee. The author finds the term "Muskogee" more appropriate than the English derived "Creek." First, he synthesizes Muskogee intercultural experiences during the two centuries before the "sacred revolt" led by the Redstick prophets. Since the author sees this groundwork as fundamental, he devotes five out of eight chapters to it. Through this reconstruction, Martin informs us about the Redstick millenarians and Muskogee intentions in 1813 and 1814. Against the relief of this historical backdrop, the prophetic revolution is illuminated clearly.

One of Martin's major contributions to our understanding lies in linking the Redstick revolt "to the

long history of Native American religious movements in which religious creativity and cosmological hope were central" (p. 177). For the Redsticks it was necessary to renounce Anglo-American lifeways, destroy symptoms of the alien culture, withdraw to sanctified ground, and renew themselves spiritually through sacred rituals (including a dance introduced by Shawnee emissaries in 1811). As Martin explains, it is important to remember that for the Muskogee new lights, hope was an operative word because they did not believe they were doomed; they were choosing a better way. Indeed, armed conflict was not inevitable because the new Redstick order would remove the faithful from corrupting influences both inside and outside the tribe. Only after the expansionistic territorial settlers precipitated the Battle of Burnt Corn did war seem inescapable. Yet even then there was hope; the prophets assured the Redstick faithful that their spiritual power would protect them. Although ultimately the prospects of the Redsticks were drowned in the bloody waters of the Tallapoosa River surrounding the sacred town of Tohopeka, the Muskogees survived; some took refuge among the Seminoles, many suffered forced removal across the Mississippi (one group reportedly carried coals from the sacred fire at Tuckabatchee to a new one at Tookafabotcha), and a few remained in Alabama. Today their descendants live throughout the United States and the world.

Working from the viewpoint of the new scholarship represented by James Merrell and Peter Wood, Martin has recovered part of a previously lost world. Through his careful rereading of the sources, he has given us a more balanced perspective about the Muskogees. Thus, he provides the opportunity for scholars to uncover details about Redstick visionaries and Muskogee leaders, such as William Weatherford. Although Martin never mentions Weatherford, Benjamin Griffith has described Weatherford as a leader of the "Red Sticks" (*McIntosh and Weatherford: Creek Indian Leaders* [1988]). In this instance and many others, Martin has challenged us to return for a second look, at the same time providing a better lens through which to focus. He is to be congratulated for this accomplishment and urged to continue his imaginative research.

JAMES H. O'DONNELL III
Marietta College

MARTHA ROYCE BLAINE. *Pawnee Passage: 1870-1875*. (Civilization of the American Indian Series, number 202.) Norman: University of Oklahoma Press. 1990. Pp. xvi, 345. \$27.95.

The postcontact experiences of the Pawnee on their traditional domain in modern Nebraska, Kansas, and South Dakota have been the subject of serious but dissimilar interpretations in the published works of Gene Weltfish, George A. Hyde, George B. Grinnell,

Clyde A. Milner, and Richard White. Added to these findings are the ethnohistorical offerings of John B. Dunbar, James R. Murie, and Martha Royce Blaine, and the superb prehistoric work of Waldo R. Wedel. Now, with an eye for more fully understanding the tragic dimension of the Pawnee's last days prior to final dispossession of their 23.5 million-acre domain in exchange for less than three hundred thousand acres south of the Osage reservation in Indian Territory, Blaine focuses on the *we-tuk* (frightful) years of 1870–75, or what might better be termed the Pawnee "trail of tears."

Eschewing Richard White's "collapse into dependency" thesis vis-à-vis the more orthodox view of non-Indian land avarice and government duplicity in the proper enforcement of alleged humanitarian policy, Blaine presents an unpleasant portrait of forced Pawnee removal, principally from the Pawnee's perspective. In this she relies heavily on personal notes and tape recordings of her late husband, Garland James Blaine, a Pawnee who was reared by his grandparents, Wichita and Effie Blaine, who were born on the Nebraska reservation and spoke no English. Garland himself spoke no English until age twelve and learned the ways of his people and how to become a chief like his people before him in his mother's (Skidi) and father's (Pitahawirata) bands. These notes and recordings provide new and important insights into Pawnee ethnography and bring into sharper focus the monumental tragedy foisted on the Pawnee during their final days in Nebraska.

In its memorial of 1870 to the Great Father in Washington, the Nebraska legislature insisted that the Platte and Loup valleys of central Nebraska could be neither developed nor improved so long as Indians resided there. It was as simple as that. The good white settlers in Lincoln worried not that the Pawnee were then experiencing malnutrition and starvation, bad treatment at the hands of the Nebraska judicial system, the virtual end of the bison supply, a serious shortage in horse mounts, and—Kevin Costner to the contrary—a seemingly calculated failure on the part of the federal government to curtail repeated assaults on less numerous Pawnee hunters and villagers by diverse Lakota groups from the north and west. Clearly the Pawnee were fair game for all.

Under profound cultural duress and with not a little economic manipulation on the part of the invader, the principal tribal leaders conceded that removal was the only alternative. Social disorganization was profound, even in the eyes of well-meaning Quaker agents, and although a surface view suggested the trek to Indian Territory was voluntary and legal, the fact remains that the government "programmed the outcome by allowing devastating conditions to exist" (p. 233). Like so many tribes that had been removed before them, the Pawnee needed to believe that they had consented, that they had shared in the spoils of white "civilization," that they understood that the process was for their own good.

By utilizing white and nonwhite sources on the dynamics of tribal displacement at the critical decision-making levels, and for the limited but important time these decisions bore fateful fruit for the Pawnee, this monograph adds an important dimension to our understanding of Indian removal in general.

WILLIAM E. UNRAU
Wichita State University

NANCY RASH. *The Painting and Politics of George Caleb Bingham*. New Haven: Yale University Press. 1991. Pp. x, 286. \$35.00.

George Caleb Bingham, the Missouri artist, was active as a painter from the 1830s through the 1870s. He and William Sidney Mount established genre painting—the realistic portrayal of ordinary people in scenes from everyday life—in the United States. American genre painting is rather more exotic, regional, and political, and certainly more centered on masculine activities, than its typical Dutch antecedents of the seventeenth century. It was Bingham's paintings of traders and boatmen on the Missouri and Mississippi rivers and of political campaigns and elections that gave genre painting a distinctive flavor that was widely hailed as American.

Nancy Rash's book is about the relationship between Bingham's paintings and his politics, for Bingham was a politician as well as an artist. He began as a Whig, then sided with Democrat Thomas Hart Benton, and in 1856 joined the Republican Party. He held both elected and appointed office for many decades. Time has enhanced his reputation as a national artist but obscured his importance as a politician. In her account the author restores the balance between Bingham's two careers. As she readily acknowledges, she is not the first to examine the political content of his paintings, but her book provides the most consistent and detailed interpretation of them in relation to his life as a politician. Her more particular contribution is the recovery from archives of such materials as Bingham's speeches and newspaper articles and photographs of paintings subsequently destroyed.

In considering Bingham's career the author includes and interprets extant, proposed, and destroyed portraits, landscapes, and history paintings, as well as the better-known genre paintings. Perhaps most interesting are the interpretations of the river paintings, which the author relates specifically to Bingham's election banners for Henry Clay and to Whig demands for federal action to make the rivers safe for commerce, and two late paintings entitled *Martial Law or Order No. 11*, in which Bingham protested against the abuses of federal power in Missouri during the Civil War. In the latter case Bingham's involvement was personal, because it was his studio that, when used as a prison, collapsed and killed five female relatives, provoking the Lawrence

Massacre. Brigadier General Thomas Ewing, Jr., issued an order for civilians to evacuate the border counties, which then were looted and virtually destroyed by Union soldiers.

There is much else of interest: the patronage of the American Art-Union, the distribution of the images through prints and photographs, the relationship between regional and national subject matter, and the differing perceptions of eastern and midwestern critics. One could wish that the book began with fewer mixed metaphors and less hype, and that its chronology were clearer so that the reader immediately had the grasp of the subject that comes only with patient reading. The author convincingly elucidates both Bingham's politics and paintings and provides an interesting and unusual instance of the relationship between the two. The book should be of interest to students of American history, American studies, and art history.

MARLENE PARK

John Jay College of Criminal Justice

DAVID ZAREFSKY. *Lincoln, Douglas, and Slavery: In the Crucible of Public Debate*. Chicago: University of Chicago Press. 1990. Pp. xiv, 309. \$34.95.

This book by David Zarefsky, a professor of speech communications, is a thoughtful and stimulating exploration of the argumentative strategies of the debates between Abraham Lincoln and Stephen A. Douglas. He argues that Lincoln and Douglas deployed four strains of argument. They both claimed conspiracy—Douglas thought Lincoln was plotting to bring abolition to the major political parties while Lincoln believed Douglas secretly hoped to spread slavery throughout the whole nation. Both also developed complicated legal claims that moved from the specifics of Dred Scott to the question of judicial review and on to the issue of states' rights. History was a third source of contention. Douglas portrayed Lincoln's "House Divided" speech as a dangerous assault on past practice. The founders, Douglas asserted, wanted the nation to survive half slave and half free. Lincoln, however, had his own historical claim, asserting that Douglas's notion of popular sovereignty in the 1850s subverted the great message of freedom in the Declaration of Independence. Finally, both made moral arguments. Douglas's morality was procedural, residing in the practice of local self-government. Lincoln's morality was substantive, grounded in a firm belief that slavery was morally wrong.

The level of analysis is not done justice by this brief summary. Zarefsky's parsing of the four strands is careful, precise, and always persuasive. Even more ingenious is how Zarefsky sees these arguments relating to each other. Explicit moral claims were dangerous, he notes, because there was no common ground to mediate them. As a consequence, both debaters

shied away from overt discussion of basic values. Instead, because both men revered the constitution and nation, legal and historical arguments came to the fore, serving as "surrogates" for the deeper ethical disagreements. One of the most important lessons to learn from the debates, according to Zarefsky, is how such "surrogate" debates allow discussion to continue despite incommensurable moral disagreements. When faced with sharply divergent moral positions (on abortion, for example), substantive debate can occur when framed around something that both sides accept (that the constitution should be the law of the land).

I have one quibble with this generally excellent work. Zarefsky rightly sees argument as the middle ground between scientism and irrationalism. But his careful dissection of the debates robs them of their excitement. In Zarefsky's hands the debates become bloodless, a calculated set of claims and counter-claims. The famous exchanges of 1858 wind up sounding very much like what I was taught to do in high school debate. This misses the public drama, the aesthetic, indeed sensual delight in the sound of words that was available to nineteenth-century speakers and audiences. The Little Giant was a party pol, given to hard words and plain speaking. Lincoln was a high romantic, with flights of language that were as much a part of his speech as the logic-chopping strategies that Zarefsky uncovers. Zarefsky misses the blur of instrumental and expressive aspects of rhetoric, emphasizing the former and ignoring the latter.

This small point aside, Zarefsky's book is a wonderful addition to the scholarship. If you believe that debate is an important value in and of itself (as opposed to simply believing your side should win in whatever way possible), then Zarefsky has said something useful about democratic theory as much as the Lincoln-Douglas debates.

KENNETH CMIEL

University of Iowa

JANET SHARP HERMANN. *Joseph E. Davis: Pioneer Patriarch*. Jackson: University Press of Mississippi. 1990. Pp. xii, 196. \$22.95.

This biography of the older brother of Jefferson Davis begins with justifications for its writing. For historians not deeply interested in the personal life of the Confederate president those justifications may seem inadequate. Janet Sharp Hermann's study does contain an interesting account of how abundant money and superb political connections cushioned the shocks of Civil War for an aging, strong-willed patriarch. But her contention that Joseph E. Davis's influence on his younger brother warrants a biography is not supported by the study she has done. And her insistence that Davis's patriarchy—real enough—was the result of his genuine affection for others is largely unpersuasive. His life may have been, as she

argues, a "quintessentially American success story" (p. x), but her evidence suggests that its theme was exploitation as much as paternalism.

Joseph Davis lived for over eighty years (1784–1870), but direct evidence about his life before 1860 is scanty. With little personal correspondence to work from, Hermann can only speculate about Davis's reaction to events, much less his influence on them. For the period between 1860 and 1870 (these ten years consume over 40 percent of the biography), when Davis did correspond frequently with his brother and other public figures, Hermann still strains to find evidence that the determined old planter's letters affected his brother's course of action in any significant way. The intent of the correspondence, quite plainly, was for Joseph Davis to acquire help in his efforts to protect his family's interests, preserve his enormous wealth, and maintain his slave labor force (he was one of the largest slaveholders in Mississippi in 1860). His determination and his physical strength are impressive in themselves, but how historically significant they were is another matter.

Hermann seeks to portray Davis as a patriarch truly devoted to those who depended on him: a large family, including three illegitimate daughters (he apparently never recognized an illegitimate son), and over 300 slaves. She does acknowledge that self-interest and affection often coincided, but most historians will infer from her evidence that the former often dominated the latter. Hermann's thesis that Davis differs from the "Oakes-Genovese model" (p. x) of paternalism in that he cared for his slaves as human beings is the most puzzling of many claims she makes about the patriarch's motives. Few of her assessments seem related to the excellent recent scholarship about southern slavery and family life.

Scholars in search of evidence about the experience of wealthy planter families during the Civil War and Reconstruction may find the details of Davis's last years useful when it is combined with information about other cases. On the whole, however, the superficial interpretations and uneven documentation in this biography do not recommend it to a wide audience.

JOHANNA NICOL SHIELDS
University of Alabama,
Huntsville

ALAN T. NOLAN. *Lee Considered: General Robert E. Lee and Civil War History*. Chapel Hill: University of North Carolina Press. 1991. Pp. xii, 231. \$22.50.

Robert E. Lee is overripe for revision. Despite some good efforts by fine scholars, the image of Lee in the American mind remains the simple, noble saint proclaimed over a half-century ago by Douglas S. Freeman in four volumes of *R. E. Lee: A Biography* (1934–35). To date the most perceptive challenge to Freeman's Lee has been Thomas L. Connelly's *The*

Marble Man: Robert E. Lee and His Image in American Society (1977). The bulk of Connelly's book traced the manipulation of the Lee image during the years after Lee's death in 1870. Because Connelly contended that Lee's image has borne pale relation to Lee's life, he concluded his book with a long chapter offering revised ideas regarding Lee and reality. Along with Barbara L. Bellows, Connelly amplified and extended this analysis of Lee in *God and General Longstreet: The Lost Cause and the Southern Mind* (1982). Connelly and Bellows spoke of Lee as "the greatest paradox of a paradoxical South," as "an exaggeration of an exaggerated society," and as "an aberration of the society that ennobled him." They concluded, "Once enshrined, his nature has become obscured."

This lengthy introduction is the minimal context in which to consider Alan T. Nolan's work on the general. Nolan's goal is "to analyze certain aspects of the Lee tradition, to consider whether the mythic Lee is the real Lee, and to evaluate Lee and the events of his life in a larger philosophical context" (p. 8). In so doing Nolan claims to employ "conventional techniques of the historian," to examine "the mythic Lee in a conventional historical way," and to expose Lee to "the light of the historical record" (p. 8). Nolan selects what he believes to be the six crucial components of the Lee myth and offers evidence that Lee in life was at odds with Lee in legend.

According to Nolan, Lee embraced slavery in practice, even though he believed the South's "peculiar institution" wrong in the abstract. Lee was very much a white southerner in 1860–61 and seceded from the United States Army (resigned his commission) for the same reasons that other white southerners seceded from the United States. Lee the Confederate general wasted southern blood and treasure in an audacious, offensive strategy that precluded any rational chance of victory. Nolan also contends that Lee the soldier hated his enemies as much as or more than any other soldier. Lee knew the war was lost long before Appomattox, but continued the carnage out of his contorted sense of honor. And Nolan's Lee after the war remained as unreconstructed as did most white southerners.

Lee, writes Nolan in his conclusion, became a mythic hero in a myth history of the American Civil War and Reconstruction. He became so because Americans need such a Lee to justify the folly of such massive bloodletting to so little purpose.

Nolan attempts a great deal in a small space. His "larger philosophical context" depends heavily on Gaines M. Foster, *Ghosts of the Confederacy* (1987), and less on Eric Foner, *Reconstruction: America's Unfinished Revolution* (1988) and other postrevisionist historians of Reconstruction, the Lost Cause, and the New South.

Lee is presented very much as a straw person who yields quite easily to "conventional techniques of the historian." Nolan focuses "the light of the historical record" in such a way as to make each of his six cases

quite neatly. Maybe some of those who read this book will believe that something called "a conventional historical way" can uncover an absolute, historical Lee.

The work pretends to be more than "Son of *The Marble Man*." Nolan does advance apace the important issues Connelly raised. But Lee continues to be overripe for revision.

EMORY M. THOMAS
University of Georgia

DOUGLAS B. BALL. *Financial Failure and Confederate Defeat*. Urbana: University of Illinois Press. 1991. Pp. xi, 329. \$29.95.

Poor Christopher Memminger! The unfortunate Confederate secretary of the treasury catches the blame from Douglas B. Ball for everything except shooting Stonewall Jackson, and the reader is not so sure that he did not have a place even in that. Despite the oversimplification of Memminger's role, Ball has provided a controversial, provocative, and generally well-researched book. It contains some serious theoretical flaws but is not to be taken lightly.

Ball's thesis is that financial failure by far proved to be the central element in the defeat of the Confederacy. He specifically argues—as have many before him—that the refusal of the Confederate government to levy direct taxes and its silly pursuit of a cotton embargo, coupled with its bungled or inadequate attempts to acquire foreign loans, constituted the major components of southern financial failure. But he perceptively points out other areas in which the Confederates were inept, including a critical inability to organize and centralize the private printers and to bring them under government control. Ball methodically traces each Confederate bond issue and carefully assesses every tax policy, finding virtually all to fall far short of providing the necessary revenue. More often than not, he notes, their policies contradicted or nullified other policies. Never, he contends, did the Confederacy seriously attempt to enlist properly the slaveowners who stood to be the prime beneficiaries of a successful rebellion. In short, Ball sees as the South's greatest weakness its unwillingness to get the planters to act on their rhetoric. He concludes with a section on an alternative hypothetical policy and an appendix outlining how the South could have won.

While clearly Ball wants to refute the view presented in Richard Beringer *et al.*, *Why the South Lost the Civil War* (1986), which severely underplayed the financial component of Confederate defeat, he often goes too far in hypothesizing what might have happened under a different program. For example, he contends that his counterfactual policies would have so materially improved the southern armies that they would have "won more victories in the field, and [those policies] would have left the South in the fall of

1864 in fighting trim" (p. 261). Not likely. By ignoring the battlefield developments, military leadership, diplomatic efforts, and a host of other factors, Ball simplifies beyond belief. His own strategy calls for retreating to defensible enclaves, against an enemy that had control of the sea, the ability to establish communication and rail lines, and who added to its military or field construction ranks with every newly freed slave. The South could only get smaller and weaker.

If readers do not allow themselves to get caught up in Ball's counterfactuals, a few real gems remain, although some of the work constitutes a rehashing of James F. Morgan's *Greybacks and Gold* (1985), which Ball never seriously addresses. More troubling still, Ball's use of the economic literature is cursory; he shows no awareness of theories of money (relevant to his discussions of currency) advanced by Thomas Sargent, Neil Wallace, or Charles Calomiris. Likewise, he apparently is unfamiliar with important works on the finance of the American Revolution by Calomiris, Bruce Smith, and Ron Michener. Finally—and the most glaring weakness—he ignores recent important analyses of Confederate money and financial policy by Gary Pecquet, who has produced a challenging approach to Confederate financial policy (in the Durrell Foundation papers) by focusing on the effects of the Union's policies on southern finances, especially the Emancipation Proclamation that undercut the value of all southern banknotes. In a number of papers, particularly one with Gregory K. Davis ("Interest Rates in the Civil War South," *Journal of Economic History* 50 [1990]), Pecquet has revised much of the debate on Confederate financial policy. Ball's omission of these and other recent works deprives the book of many of the important nuances it needs to refine its inexorable damnation of Memminger and the Confederate government.

Make no mistake, though. This is an important book, even with those weaknesses, and should challenge a new round of revisions. Certainly one would expect Memminger's relatives to write a letter to the editor!

LARRY SCHWEIKART
University of Dayton

MARK E. NEELY, JR. *The Fate of Liberty: Abraham Lincoln and Civil Liberties*. New York: Oxford University Press. 1991. Pp. xvii, 278. \$24.95.

It is well known that during the first weeks of the Civil War, President Abraham Lincoln authorized suspension of the writ of habeas corpus to protect the passage of Union soldiers to Washington, D.C., and to smother the possible secession of Maryland; that by fall 1862, Lincoln had extended this suspension geographically to the nation and proclaimed all who interfered with military mobilization or aided the enemy subject to martial law; and that the Lincoln

administration declared martial law in certain insurgent areas, held civilians without trial or tried them before military commissions, shut down several newspapers temporarily, and arrested the "fire-eating" peace orator Clement L. Vallandigham after an incendiary speech and exiled him to the Confederacy.

Mark E. Neely, Jr.'s welcome book on civil liberties under Lincoln advances us beyond this sketchy profile to detail the workings of Lincoln's internal security system. Neely reviews voluminous military court and federal prison records that have not heretofore received systematic attention, encompasses the perspectives of president and cabinet as well as those of hundreds of officials in the field, and refines the terminology and conclusions of the old authority, James G. Randall's *Constitutional Problems under Lincoln* (1926). The result is a balanced and compelling account that fully acknowledges and explores repressive practices, but repeatedly emphasizes the limits observed by Lincoln and the wartime bureaucracy. Although there were many more arrests than the approximately 13,000 estimated by previous historians, the federal military apparatus was seldom used to suppress political opposition on the home front—the Vallandigham case was an exception. After 1862 most of those arrested were Confederate citizens. Arrests north of the border states were relatively few, often involved desertion and draft evasion, and only rarely acts of speech, writing, or assembly. "Though far from legally correct," military trials in the guerrilla districts of Missouri were not "show trials of predetermined outcome" and were characterized by regularity, routine procedures of review, and some attention to the rights of the accused (pp. 42–43).

Not incidentally, Neely, the Director of the Lincoln Museum, seeks to buttress Lincoln's "reputation" by putting to rest the recurring charge of dictatorship. No doubt he succeeds. But the book suffers somewhat from the lack of a clear definition of "liberty" and of a thoroughgoing discussion of how mid-nineteenth-century Americans understood freedom, of the sort initiated in Don E. Fehrenbacher's essay, "The Paradoxes of Freedom" (in *Freedom in America: A 200-Year Perspective*, ed. Norman A. Graebner [1977]). The absence of such a definition and discussion gives the debate over Lincoln's "reputation" a vague and anachronistic quality. In general, Neely is more adept at describing the treatment of citizens by the wartime government than at moving out into the broader culture to explain why the peculiar mixture of martial law and restraint obtained. That mixture calls to mind Gordon Wood's observation that in the "manipulative tradition of American law . . . the peculiarly American practices of judicial review and [mob] vigilantism were actually two sides of the same legal coin" (Bernard Bailyn *et al.*, *The Great Republic* [1977], 290–91). Military arrests and trials may have made similar sense to some nineteenth-century Americans as part of that "tradition" of orderly extralegalism. In

America, the people could make law—and if need be, by-pass constituted authority—as exigencies required.

IVER BERNSTEIN
Washington University
St. Louis, Missouri

DAVID WARREN BOWEN. *Andrew Johnson and the Negro*. Knoxville: University of Tennessee Press. 1989. Pp. xvi, 206. \$29.95.

Recent historians have attributed Andrew Johnson's problems with Congress to political ineptitude by an outsider. In this fine analysis of Johnson's thought, David Warren Bowen suggests that "racism is the 'key' to understanding Johnson's actions and . . . Reconstruction itself" (p. 166). The author explains racism as a complex set of beliefs that must be placed in the context of an individual personality to appreciate its meaning and effect on society. In general, racism is a belief system that requires society to be divided by inherent group differences.

Bowen argues that there was an underlying consistency to Johnson's racial attitudes, although his ideas, especially about slavery, changed over time. Johnson lived in an era when holding racist ideas about people who looked distinctly different was almost unavoidable. Most white Americans believed in a racial hierarchy, with whites at the top and blacks at the bottom. Basing his argument on John Dollard's theory in *Caste and Class in a Southern Town* (1949; 1988), Bowen maintains that middle-class whites had more antipathy for African Americans than lower-class whites. Blacks represented a greater threat to the status of middle-class whites, whose position in society was tenuous, who aspired to join the upper class but who could easily slip downward depending on economic fortunes, and who therefore sought a floor to stabilize the position of all white men.

Johnson had recently ascended to the middle class. His parents had been servants in Raleigh, North Carolina, where he was apprenticed at age fourteen to a local tailor. Illiterate and unskilled, he worked diligently to improve himself before moving to Greeneville, Tennessee, where he set up shop, educated himself, acquired considerable property, and entered politics. Influenced by the Jacksonian ideal that every man was as good as the next man, he became a champion of the "masses." Johnson believed that men should be judged by their accomplishments, not by their birth. For him, democracy included only the true members of the family of man. Blacks could not enjoy the benefits of democracy or equality because they did not belong to the family of man. To admit them into the democratic system would destroy the social equality that white men shared among themselves.

Johnson assumed that slavery sustained white supremacy. Although he later supported emancipation

as a means to preserve the Union, he remained steadfast in his commitment to white supremacy. He was rigid in his racial attitudes. Whereas some politicians made a distinction between "natural" and "political" rights, Johnson made no such distinction. He declared that a man was equal or he was not. Political participation in a Jacksonian mode gave men a status that made them all equal. To give black men the ballot would make them equal to white men, a prospect that Johnson opposed even at the risk of impeachment.

Although Johnson's contemporaries (moderate, radical, and reactionary) harbored racist attitudes, his beliefs were much more inflexible, involving his own status and that of the average white man. Bowen has done a magnificent job in distinguishing among politicians with racist beliefs, whose patterns of thought meant little until it was time to act. Johnson appeared contradictory and was misunderstood by most of his contemporaries. This study gives us better insight into the fundamental ideas that guided his actions.

ROBERT L. HARRIS, JR.
Cornell University

RODNEY D. OLSEN. *Dancing in Chains: The Youth of William Dean Howells*. (The American Social Experience Series, number 20.) New York: New York University Press. 1991. Pp. xxiii, 344. \$45.00.

Like many nineteenth-century middle-class males, William Dean Howells suffered from a spate of psychological infirmities. Bouts of vertigo, debilitating self-doubt, and paralyzing fear of premature death haunted both his youth and much of his later life. The virtue of Rodney D. Olsen's book is that it conveys the broader cultural significance of these ailments. Drawing on insightful analyses of Howells's correspondence, early literary efforts, and several reminiscences of life in Ohio, Olsen links the psychic turmoil of the nation's foremost literary realist to the tensions inherent in the reorientation of family life from adherence to communal expectations and patriarchal decree to preparation of offspring for individual achievement through careful nurture.

Olsen rightly does not claim that Howells had a typical mid-nineteenth-century boyhood. Few adolescents had fathers with such unswerving Swedenborgian loyalties (although Howells's later confidant Henry James, Jr., did). Yet William Cooper Howells's philosophical teachings paralleled other earlier forms of thought in their understanding that the moral order that pervaded the universe could be perpetuated only through selfless communal service. Imparted to the younger Howells through the "force of affection" rather than corporal punishment, that legacy existed uneasily with the emerging middle-class success ethic. Designated as the family hope and, unlike his siblings, excused from providing for the family maintenance, William Dean internalized the

tensions between the two ideals as a life-long struggle to avoid the pitfalls of "selfishness" intrinsic in any expression of individual desire. The conflict became particularly acute for Howells at moments of home leaving, when independence from family raised the specter of sinfulness.

Howells managed an emotional truce by pursuing a literary career predicated on usefulness. Literary service was not easily accomplished in an environment where the marketplace frustrated literary leadership at nearly every turn. Howells took refuge in Heinrich Heine's example of the poet whose unique gift separated him from society and compelled him to serve progressive ends, but Howells eventually found that both the subjectivity and the absence of a morally ordered world entailed in such a view clashed with his father's Swedenborgian injunctions. Olsen astutely traces how these tensions shaped Howells's early gropings toward a realism that adjudicated the competing impulses of usefulness and individuality.

Olsen's treatment of the mature William Dean Howells is less deft. Although he hints at the youthful roots of Howells's breakdown in 1881 during the writing of *A Modern Instance*, Olsen does not entirely indicate why the repressed feelings returned. Nor does Olsen's reading of *A Hazard of New Fortunes* (1890) as a revival of the old conflict fully register Howells's newfound knowledge of the growing intractability of social processes. One may also quarrel with Olsen's implication that Howells's failure to join his protégés amid the "foul and trampled slush of the common avenues of life" (p. 273) signifies his literary timidity; what Frank Norris termed the tragedy of the broken teacup was often bolder and more acute than later critics acknowledged. These minor shortcomings do not, however, overshadow Olsen's valuable demonstration that Howells's innovations emanated from his working through the emotional ramifications of the cultural dilemmas of his age.

DANIEL H. BORUS
University of Rochester

STEVEN SEIDMAN. *Romantic Longings: Love in America, 1830-1980*. New York: Routledge. 1991. Pp. x, 247. \$25.00.

In his study of the sexuality of white, middle-class Americans, Steven Seidman brings to the historical evidence a sociologist's perspective. His goal is to construct "a cultural analysis of love" (p. 2). In analyzing what he describes as "mainstream public meanings" (p. 5), he leans heavily on advice literature and popular medical texts, supplemented by a selective look at diaries, letters, autobiographies, and surveys of sexual behavior. His endnotes reveal a familiarity with the recent historical literature.

Seidman describes three broad constructs of love and sex among the middle class. The Victorians (1830-90), who were neither as repressive as older

stereotypes suggest nor as expressive as some revisionists claim, spiritualized love. Because sex posed a danger to the essentially spiritual character of marriage, Victorians sought to desexualize love and to desensualize sex. When controlled, regulated, and properly channeled, the threatening aspects of sex could be minimized and sex would occupy its proper place in a union of spiritual companions.

The modern era (1890–1960) saw the antithesis between love and sex, and between love and sensuality, disappear. According to Seidman, this “sea change” (p. 78) occurred in the context of a perceived crisis in sexual morality and marriage in the early twentieth century. Popular writers and social science experts recast sex as an essential aspect of marriage and sexual maladjustment as the chief cause of marital unhappiness. Sexual expression assumed vastly greater importance; marriage became a matter of companionship and sexual pleasure.

By legitimating eroticism, the modern era opened the door to the latest shift in sexual meanings. In the contemporary period (1960–80), Seidman detects something decidedly new: the valorization of the erotic aspects of sex apart from marriage and, indeed, apart from love. Sexual pleasure has come to be its own justification. Seidman acknowledges that, despite the appearance of these new discourses, a consensus as strong as that of the Victorian and modern eras has not emerged. Rather, contemporary life is characterized by a sharp contestation between romantics and libertarians over sexual meanings.

Although historians of sexuality will find the overall argument familiar, there are distinctive features. His postrevisionist treatment of the Victorians contains not a touch of moralism. These Americans are neither prosex nor antisex, neither liberated nor repressed. Seidman also skillfully integrates discussions of same-sex relationships into his argument; this is particularly true of the chapters on the contemporary era in which gay experience figures prominently.

I have reservations about some aspects of his argument. In his effort to identify dominant sexual meanings, Seidman ignores crosscurrents and counter-meanings, even within the middle class. He strives too much to homogenize, particularly for the contemporary era in which he deemphasizes the continuing salience of gender and minimizes differences between, on the one hand, gay male life and, on the other, both heterosexual and lesbian life. I was also puzzled by his focus on radical feminists as dissenters from the ethic of a nonromantic sexuality. Surely the New Right, which is missing from these pages, has offered a sharper, and more politically contentious, critique.

Despite these reservations, Seidman offers a useful and engaging study detailing the meaning of love and sex within the white middle class.

JOHN D'EMILIO
University of North Carolina,
Greensboro

NANCY GREY OSTERUD. *Bonds of Community: The Lives of Farm Women in Nineteenth-Century New York*. Ithaca: Cornell University Press. 1991. Pp. ix, 303. Cloth \$42.50, paper \$14.95.

Nancy Grey Osterud tells the story of kin and community life in the Nanticoke Valley of upstate New York from 1787 to 1900, using a fascinating and exhaustive collection of quantitative and literary sources. For this close, careful, and thoughtful work alone, this book would be worth the read.

Osterud, however, has taken on the larger task of arguing that the women of this rural valley, realizing their gender-imposed limitations, worked to improve their condition not—as Carroll Smith-Rosenberg and Nancy Cott have argued—by turning away from men to a separate world and culture, but “by actively enlarging the dimensions of sharing in their relationships with men” (p. 274). Osterud attends to gender-based limitations on women’s lives, but she focuses on the family, kin, and communal bonds that connected genders. Working out a practice that she labels “strategies of mutuality,” she contends that women “stroved to create mutuality in their marriages, reciprocity in their performance of labor, and integration in their modes of sociability” (p. 275). In this way, these rural women enlarged their own opportunities, improved their own lives, and actively worked against the hegemony of the burgeoning urban, industrial, capitalist, and sexist culture of the nineteenth century.

Osterud asserts that women consciously adopted these strategies, but the evidence for their self-consciousness is slim. On one level, the fact that they acted in this way is enough, but on another level it makes an enormous difference whether the women were conscious or not. If these women intentionally worked against the oppressiveness of their lives, were they not acting out of the most fully developed women’s culture? Somehow consciously applied “strategies of mutuality” suggest that these women had fairly well-developed feminist understandings of gender and demonstrate the vitality and power of the women’s culture that Osterud is putting to the test. This point deserves to be argued and explored.

Although mentioning the Irish immigrants and those who moved on, Osterud emphasizes those who stayed behind. Her undertreatment of various outsiders re-creates the dominant culture’s own definition of their valley and of who and what mattered: Anglo, Protestant, and harmonious. What about those Irish? What about discord and disharmony? What about kin lines that carried animosity and ill will? What about the transients, the poor, the outsiders, the disruptive? Maybe these people are just too hard to find, or there are just too few records. Osterud does mention men who went west and never returned, women who found refuge with family members when abused by a husband, but these stand as the examples that make me wonder even more about what these people did not tell, about what price

harmony demanded, and who got squeezed out and overlooked and ignored. Would more attention to these centrifugal forces not create a somewhat different portrait of community life?

These are questions for thought, not criticisms of this fine book. Osterud's work deserves careful attention from historians of women, rural life, and nineteenth-century America. Her graceful writing and common-sense approach and presentation make the book a pleasure to read.

ANNETTE ATKINS
St. John's University
Collegeville, Minnesota

PAULA BAKER. *The Moral Frameworks of Public Life: Gender, Politics, and the State in Rural New York, 1870–1930*. New York: Oxford University Press. 1991. Pp. xviii, 251. \$29.95.

Paula Baker has written an important book. In it, she analyzes the interrelationships among gender, politics, and the state, and plants those relationships securely in the economic, geographic, and religious fields of rural New York between 1870 and 1930. In its simplest form, Baker's thesis is that men and women developed separate political traditions during the nineteenth century, that in the early twentieth century the two merged, and that by the 1920s politics no longer provided an arena in which women and men enacted their gender identities.

Baker is at her best when analyzing the late nineteenth century. Then, rural New Yorkers considered participation in electoral politics a part of manliness. Manhood embraced such traits as integrity, loyalty, independence of mind, industry, and friendship, and the rituals of partisan politics allowed men to demonstrate these qualities. In fact, however cynical rural New York men might have been about the corruption of politicians and government, they remained intensely partisan in the late nineteenth century and continued to believe that electoral politics—through granting offices and patronage jobs—rewarded true manhood. They reconciled their cynicism with their moral expectations by hoarding power in their local communities and restricting the more distant state government.

Excluded from electoral politics, women affected public policy through various forms of lobbying and direct action. They succeeded especially in the areas of temperance and women's rights, and indeed excelled in nonpartisan, single-issue politics. Unlike men in their communities, female activists considered the state government a potential ally in their righteous causes and consequently advocated a more activist government than men did.

According to Baker, a variety of trends—including the activities of state agencies created by urban progressives and the victory of women's suffrage—converged after 1900 to encourage rural New York men

to take up the political traditions of their womenfolk: increasingly, complete cynicism or interest-group politics replaced intense partisanship. In this significant retelling, then, women's nineteenth-century political traditions were the wave of the future for American politics.

Baker's greatest contribution lies in her demonstration of the ways in which nineteenth-century notions about manhood and womanhood structured American political life. She has established the intimate relationship between gender and politics in the nineteenth century; she has confirmed the necessity of putting women and gender at the center of political history.

What she leaves for others to explore is how manhood and womanhood themselves were thrown into crisis and remade by the political changes that wracked the twentieth century. Exploring this dynamic might have strengthened Baker's attempt to comprehend the contradictions of politics in the 1920s. Nevertheless, the new feminist literature that investigates this dynamic and demonstrates the continuing inscription of gender in public policy throughout the twentieth century needs this opening chapter by Baker. Her work is a crucial part of an exciting reconception of the relationship between state and society at the turn of the century.

ROBYN MUNCY
University of Maryland,
College Park

JAMES P. RONDA. *Astoria and Empire*. Lincoln: University of Nebraska Press. 1990. Pp. xiv, 400. \$25.00.

Here is complex history presented with admirable clarity. James P. Ronda asserts that Astoria in the nineteenth century was both a place and an idea and that, of the two, the idea was longer-lived and of greater importance. More than simply the site of a grand American adventure first imagined by Washington Irving and memorialized in subsequent accounts for more than a century, the fur-trading outpost was the focal point of a vast multinational competition for sovereignty in the Pacific Northwest. Irving merely hinted at a broad struggle for empire, and then only in the last sentences of his *Astoria* (1836); but until now there has been no thorough discussion of the multiplex ramifications of the Astorian experience.

Astoria's frontier context may lead some to anticipate at least a nod to Frederick Jackson Turner's antecedent theme of place-as-process. But Ronda manages nicely without Turner or, as it happens, any other theorist of frontier or West, including the recent crop of dogmatic neo-regionalists who deplore economic development of any sort if it contributes to America's "legacy of conquest"—and that means just about everything. *Astoria* is an unhandy vehicle for latter-day polemicists because it was not, either as

place or idea, the exclusive property of white males, a point about which Ronda seems most emphatic. There may have been Yankees, Russians, French Canadians, and Scots aplenty in Astoria and environs; but Hawaiians surnamed Paou, Kimoo, and Powrowie; the Iroquois hunter Ignace Shonowane and his family, the Chinook chief Comcomly, and Clatsop leaders Coalpo and Daitshowan were among dozens of individuals whose presence demonstrated the remarkable ethnic diversity of the place. The exposition of this alone may be enough to encourage politically correct "new" historians of the American West to claim Ronda as their own. How his discussions of sexual economics and social disease will square with current concerns, however, one may only imagine.

The bibliography is impressive but occasionally flawed. Editorial inattention makes it appear that Savoie Lottinville edited Thomas Nuttall's 1819 journal in 1821 (which some, for various reasons, might be inclined to believe) and that Edgar Stewart and Jane Stewart edited Ross Cox's *Columbia River* in 1831. Otherwise, the volume seems largely free of typographical distractions. It is always gratifying to be able to note that a useful and important book is handsomely made and priced within reason.

WILLIAM W. SAVAGE, JR.
University of Oklahoma

LARY M. DILSAVER and WILLIAM C. TWEED. *Challenge of the Big Trees: A Resource History of Sequoia and Kings Canyon National Parks*. Three Rivers, Calif.: Sequoia Natural History Association. 1990. Pp. xii, 379. Cloth \$24.95, paper \$14.95.

In recent years, historians of the national parks have come to realize that the standard research format—focusing on legislative and administrative themes—was seriously deficient. Instead, it was recognized, a more serious, critical review of the evolution of the national parks as biological reserves was needed. This book by Lary M. Dilsaver and William C. Tweed is written with that wider focus. Beyond recasting the familiar history of Native Americans, explorers, settlers, and the legislative battles for Sequoia and Kings Canyon national parks, the book asks: "Can a modern society preserve 'unimpaired' not just isolated natural features, but entire natural ecosystems while at the same time intensively using them for recreation?" (p. ix)

The answer is a vivid and absorbing environmental history of these two great national parks. The majority of the volume is drawn from long-ignored primary sources, including and especially the official letters, reports, and memoranda of park biologists and government research scientists. Accordingly, there is much that is new here. Yet in the interest of comprehensiveness the authors do not ignore the familiar history, either. Nonetheless, what sets this book apart

is the concentration, as promised, on the issues of biological conservation and resource integrity.

Invariably, as one result, the book provides a detailed examination of the career and opinions of Colonel John Roberts White, the outspoken superintendent of Sequoia National Park from 1920 to 1938, and again between 1941 and 1947. White's running battles with the park concessionaire are colorfully told, as are the consequences of the concessionaire's successful defense of his facilities within Giant Forest. Summing up his career, White wrote William Colby of the Sierra Club, "during my 28 years in the national parks the concessioner problem was the only really difficult one, largely insoluble by a superintendent because the concessioner had such constant access to higher ups of the Department and the Service and could afford to disregard a superintendent" (p. 171).

Unfortunately, the more a concessionaire successfully nibbled away at a park, the more likely its biological problems steadily worsened and multiplied. By the 1960s and 1970s, the effects of increasing visitation, traffic congestion, and the building of park structures had rippled far beyond the confines of Giant Forest. In Kings Canyon, too, dramatic increases in visitation exerted mounting pressures on park wildlife and natural areas. It still might have been worse, we are reminded, if Superintendent White and his supporters had initially given in to proposals for more highways and wilderness development.

Indeed, the book covers far more than its title would suggest, going well beyond the resource history of the giant sequoias to a comprehensive treatment of the parks' biological problems at large. Also much to their credit, the authors were not satisfied with pathbreaking research and fine writing. There are thirty-two outstanding maps, tracing the evolution of the Sequoia-Kings Canyon region from aboriginal times to the present, with special focus on the changing boundaries and resource issues of the two national parks. The book is also sprinkled throughout with well-chosen photographs. Add to that a beautiful design and an easily read typeface, and the package is irresistible. From research through production, this is a meticulous piece of work. Undoubtedly, it will guide other environmental historians with similar research interests, all the while serving as a definitive reference for the successes and failures of the national park idea in California and the West.

ALFRED RUNTE
Institute for Pacific Northwest Studies

MALCOLM J. ROHRBOUGH. *Aspen: The History of a Silver Mining Town, 1879-1893*. New York: Oxford University Press. 1986. Pp. 263. \$19.95.

Aspen, located deep in the Rocky Mountains of Colorado, was for a short time the greatest silver

camp in the United States. At its zenith this town in the Roaring Fork Valley rivaled Virginia City in Nevada's comstock country, the camps of the California gold rush, and Leadville and Cripple Creek in Colorado. Malcolm J. Rohrbough's book begins in 1879 when prospectors crossed the Continental Divide and camped on Colorado's western slope at the foot of Aspen and Smuggler Mountains.

During the early years of its development, Aspen faced enormous problems. Isolation, heavy snows that closed the camp (save for a hardy few in the winter), lack of a railroad and smelters, and litigation between mining companies kept Aspen from growing until 1883. The great era of development and expansion began in that year when Jerome Wheeler, a partner in Macy's department store, bought several claims and built roads and a badly needed smelter.

In many ways Aspen was a microcosm of the mining frontier in Colorado and the West. It began with great optimism and then an economic boom that lasted until 1893. Before then eastern investors had arrived, railroads and smelters were built, enormous fortunes were created, and a cultural metropolis in miniature appeared. By 1890 Aspen had 12,000 inhabitants, two railroads, six newspapers, electricity, an opera house, and a streetcar system. The town ranked third in population in Colorado, behind only Denver and Leadville.

Unfortunately, by the early 1890s Aspen's future was shrouded in darkness. The "battle of the standards"—gold versus silver—that had been fought since 1873, reached a climax. The price of silver plummeted to 58 cents an ounce in 1893, precipitating the infamous silver panic. Aspen and Colorado, finding little relief from either Democratic or Republican parties, turned to a third party—the Populist Party, but the die was cast.

Because of the silver panic Aspen was finished as a great mining town. The population fell to just over a thousand, smelters and mines closed, and businesses went bankrupt. After the turn of the century Aspen was just another small mining town on the western slope of the Rockies, its dreams of becoming a major metropolis destroyed by the gold standard.

By the Great Depression of 1929, Aspen was nearly a ghost town. Then a new generation of entrepreneurs discovered another natural resource that had always existed in that mining town. This resource revived Aspen, brought countless more millions of dollars than during the glory days of the past, and made the old mining town world famous. It was snow.

DUANE VANDENBUSCH
Western State College
Gunnison, Colorado

HOWARD LAWRENCE PRESTON. *Dirt Roads to Dixie: Accessibility and Modernization in the South, 1885–1935*. Knoxville: University of Tennessee Press. 1991. Pp. x, 206. Cloth \$38.50, paper \$18.95.

At the beginning of this century, only the most intrepid Yankees ventured across the Mason-Dixon line. The road south was arduous; one motoring pioneer drove his Pierce-Arrow from Litchfield, Connecticut, to Charleston in thirty-four days, using mud hooks to ford streams. Just twenty years later, thousands of Americans journeyed southward by automobile in relative comfort, motoring to the winter resorts of Florida along the Dixie, Lee, and Bankhead highways, new interstate tourist routes lined with filling stations and billboards advertising Burma Shave. Howard Lawrence Preston analyzes this remarkable transformation, the people who promoted it, and the excruciating limits of the efforts to pull the South out of the mud and lead it toward economic prosperity.

In compelling detail, Preston narrates the history of the Good Roads Movement in the early twentieth-century South: the ways profit-seeking visionaries such as Birmingham publisher John Asa Rountree and Florida developer Carl Fisher mobilized southern politicians and chambers of commerce to construct tourist highways in the 1910s and 1920s. Preston also depicts the ribbons of newly paved roads as the axis of conflict in the emerging modern South: an issue dividing agrarian interests seeking farm roads from business boosters, Populists from business progressives, ardent states' rightists from solicitors of federal aid. Preston portrays the campaign for good roads ultimately as a battle over southern regional identity, a conflict between those who hoped new highways would obliterate sectional differences and those who feared just such a result.

Although the highway builders triumphed, they won, in Preston's view, a hollow victory. The tourist routes of the 1920s transformed the small towns of the South, converting the lucky (or unlucky) communities along the highways into tourist oases while leaving outlying towns to want, squalor, and decay. Farmscapes became roadsides, agrarian reform metamorphosed into sterile business boosterism, a plastic sameness began to erase the South's distinctive culture, and, despite it all, the region received few riches in return. It remained in 1935 an impoverished, tumbledown region.

Along the route, Preston drives some conclusions too hard. He overemphasizes the agrarian and reformist origins of Good Roads Progressivism. The partisans of pavement were no wolves in sheep's clothing distorting a Populist push for rural uplift into a conservative campaign for tourist roads. The Good Roads Movement was almost entirely a creation of business progressives; southern farmers were hardly committed to constructing market roads and, as Preston shows, entirely averse to paying for them. Similarly, Preston overstates the economic and cultural impact of the early tourist highways.

Nonetheless, this fine book delineates the gestation period of a new political and economic order in the South—one that would not take firm shape until after World War II. This was the South dominated by the

advocates of economic development, a region willing to dispense with states' rights in order to pursue federal aid, one happy to betray the tradition of limited government as long as government action aided industrial development.

BRUCE J. SCHULMAN
University of California,
Los Angeles

HAROLD L. PLATT. *The Electric City: Energy and the Growth of the Chicago Area, 1880–1930*. Chicago: University of Chicago Press. 1991. Pp. xviii, 381. \$34.95.

An important book deserving a wide reading, this is one of relatively few works that successfully links technology, entrepreneurship, politics, social change, and popular culture to enhance our understanding of the impact of urban technology on everyday life. Harold L. Platt demonstrates how electrical energy affected daily life in Chicago at the turn of the century, transforming a densely packed center of population from a deathtrap to a place of convenience and luxury unparalleled in rural America.

The analysis falls into three distinct but interrelated segments: the period from 1880 to 1898, when Chicagoans searched for better forms of illumination, culminating in the development of electricity; the years from 1898 to 1914, in which city government and utility brokers confronted and overcame two obstacles: how to generate a cheap supply of electricity for every business and home and how to persuade people who were already enjoying gas services to switch to electricity; and, last, the period from the outbreak of World War I to the Great Depression, when electric power was extended throughout the metropolitan region. Commencing with the energy crisis of World War I, the author traces the birth of the machine age and the emergence of "the electric city" in the 1920s as a symbol of Americans' commitment to an energy-intensive, mass consumption, and leisure-oriented society, segregated by income, class, and race. Platt concludes with an incisive analysis of the collapse of Samuel Insull's utility empire in 1932 and of his legacy: the ubiquitous world of electricity.

Chicago is a logical choice for this case study. As the world's fastest growing big city in the 1880s, its incredible growth meant economic opportunity, which in turn attracted some of the most innovative electric men and utility entrepreneurs of the day. After Thomas Edison, the most prominent among them was Insull, whose mastery of public utility economics, central station service, and urban politics enabled cheap electric power to displace alternative forms of energy for business, industry, and the consumer. As Chicago became an energy-intensive society, it also became a "networked city." It gradually developed a new infrastructure, spilled over its geographical boundaries, and reached into the urban fringe. The city's politics, meanwhile, became more

competitive as the stakes grew higher, and its citizenry had to make more complex choices. By 1932 the "invisible world" of electric energy had irretrievably altered the social and economic fabric of the metropolis.

Platt meticulously reconstructs the early years of the electric utility industry in Chicago and Insull's pivotal role in promoting the new technology to demonstrate that contemporary social values and cultural orientations determined both the pace and direction of technical change. The rapid diffusion of electric light and power systems across the urban landscape was neither inevitable nor driven by some inherent technological imperative; it was, he contends, the result of conscious choices made by Chicago's citizens. So, too, the emergence of utility monopolies in the decade following World War I was the result of choice in the form of business decisions, public policies, and consumer preferences, not the inexorable logic of the machine.

The author's analysis and carefully documented conclusions are derived from exhaustive research into the massive historical records of the Commonwealth Edison Company of Chicago, spanning the period from 1849 to the present. Traversing several disciplines and writing in lucid, concise prose, Platt advances our understanding of technology and cultural change, and the process of urban growth and development. In doing so, he provides a model of how American urban history should be written.

PHILIP J. FUNGIELLO
College of William and Mary

RICHARD A. GARCÍA. *Rise of the Mexican-American Middle Class: San Antonio, 1929–1941*. Foreword by HENRY C. SCHMIDT. (The Centennial Series of the Association of Former Students, Texas A&M University, number 36.) College Station: Texas A&M University Press. 1991. Pp. xvi, 398.

As a prominent city in the borderlands whose history dates back to Spanish colonial days, San Antonio is an excellent center to study the forces that have affected the Hispanic way of life. In this innovative book, Richard A. García focuses on the responses of the Mexican-American middle class to the simultaneous pressures of integration into U.S. society and preservation of Mexican culture. The generation examined by García was the first statistically significant Hispanic cohort to assimilate modern American culture.

By the 1920s permanent residence in U.S. urban areas for large numbers of Mexicans had become a reality, and those above working-class status who were able to exercise some flexibility in defining their social world followed a logical strategy: they sought to balance their traditional Mexican way of life with that of Anglo America. Culturally they retained many Mexican traits, but politically they began to identify with the United States, reflecting their acceptance of

permanent residence north of the Rio Grande and their realization that group progress hinged on their ability to protect their interests. Meanwhile, upper-class exiles who had settled in San Antonio during the years of the Mexican Revolution attempted to prevent wholesale assimilation into U.S. society by promoting retention of Mexican traditions, customs, holidays, and symbols.

LULAC (League of United Latin American Citizens), the middle-class Mexican-American civil rights organization that emerged in Texas during the period, championed the concept of a dual identity. It preached the gospel of economic, political, and social integration into U.S. society, but at the same time admonished its followers to take pride in their Mexican heritage and to be bilingual and bicultural. In particular, LULAC stressed the need to master the English language and to obtain a good U.S. education.

García has written an important work that realistically and dispassionately assesses the struggles of a generation that experienced great changes from the Great Depression to the beginning of World War II. The author reveals a deep understanding of the complex nature of Mexican-American society, and of the impact of major processes such as immigration, repatriation, economic dislocation, and adjustment to urban life.

The book, rich in detail and analysis, consists of three sections. Part 1 focuses on Mexican Americans in the context of U.S. urban society; part 2 examines their participation in institutional life, that is, the family, the church, education, and politics; part 3 discusses the views, attitudes, and ideology of both middle and upper-class Mexican Americans. This book should be valuable to anyone interested in assimilation and ethnic identity.

OSCAR J. MARTÍNEZ
University of Arizona

WILLIAM H. MULLINS. *The Depression and the Urban West Coast, 1929–1933: Los Angeles, San Francisco, Seattle, and Portland*. (The American West in the Twentieth Century.) Bloomington: Indiana University Press. 1991. Pp. x, 176. \$35.00.

William H. Mullins addresses an important and neglected subject in this first comparative study of West Coast cities during the early years of the Great Depression. Doubting that histories of the federal response to the depression best illuminate the accommodation to economic and social crises in the administration of Herbert Hoover, Mullins makes a detailed study of Los Angeles, San Francisco, Seattle, and Portland, and compares their history with the experiences of other major cities outside the western United States. Favorably impressed by revisionist interpretations of Hoover's political ideology and leadership style, Mullins examines the extent to which

the president's point of view matched the beliefs and practices of city governments. Mullins also evaluates the evidence for and against the proposition that the New Deal brought dramatic changes in policy in these cities. In Mullins's view, the western cities experienced as much or even more hardship than their counterparts elsewhere; policies initiated by local leaders drew on cultural predispositions similar to those of the president, and continuity rather than change characterized the transition from the administration of Hoover to that of Franklin Roosevelt.

Mullins employs the concept of political culture, which he neither defines nor qualifies in respect to geography, class, ethnicity, or gender, to explain both the local and national response to the range of dislocations created by the depression. Characterizing Hoover's personal ideology as "cooperative individualism" in contrast to a harsher "rugged individualism" that the president repudiated, Mullins looks for evidence that those presumably national values and beliefs existed on the local level by examining a wide range of primary sources containing expressions of sentiment, as well as the content of local policies on relief and employment for the victims of the depression (pp. 3–4). Within the limitations of his methodology, the author's findings are well supported and instructive. Although they differed among themselves, with Oregon displaying more sympathy for depression victims than California and Washington, the three western state governments tended to follow "rugged individualism" to a greater degree than, for instance, New York and Pennsylvania. At the city and county level, however (with San Francisco policy showing the greatest support for the down and out), "cooperative individualism" stimulated a series of measures that in their timing and their scope differed in no significant ways from the record of non-West Coast cities.

Mullins has succeeded in assembling an important compendium of detailed information concerning the gradual development of state, county, and city government policy responses during the period from 1929 to 1933. The material is presented in a straightforward chronological narrative, with references to questions of explanation and interpretation presented at numerous instances in the text. The lack of a bibliography is annoying but perhaps an even more serious omission, one that speaks to roads not taken rather than to goals not achieved, is the author's failure to locate his useful work in the context of recent scholarship on social policy and state development.

WILLIAM ISSEL
San Francisco State University

WALTER WERNER and STEVEN T. SMITH. *Wall Street*. New York: Columbia University Press. 1991. Pp. xi, 306. \$35.00.

Walter Werner and Steven T. Smith chronicle the development of "public corporations and the influence of securities markets in the 1790 period, with a briefer discussion of how the roots put down during the first fifty years matured during the next fifty years" (p. 6). Begun by Werner, the project was completed by Smith after Werner's death.

The book unfortunately has all of the earmarks of an unfinished work. Of the 156 pages of text, 129 are devoted to the period from 1790 to 1840, and twenty-three deal with the so-called "Railroad Age," from 1840 to 1890. The book is a seemingly random, eclectic, and anecdotal treatment of this important period that shaped industrial capitalism in the United States. There is no extended examination of how Wall Street played the role of bringing together capitalist and entrepreneur or what factors prompted capital to flow in one direction rather than another. Chapters 6 and 7, on the "Purchasers of Securities" and "Issuers of Securities," might appear to hold some promise but are in such outline fashion as to be of little value. The authors, for example, spend only a page and a half on the financing of the Erie Canal. Furthermore, there is little sense of the political, economic, or social climate within which Wall Street operated.

And, while there is no sustained analysis of Wall Street in the twentieth century, there seems to be a fairly explicit right-wing political agenda informing this work. "Today's elaborate financial instruments, flamboyant manipulators and insider traders, hostile takeovers and market crashes may be understood as the flowering of seeds planted in the United States more than 200 years ago." The authors contend, however, that these "modern manifestations are merely another stage in the steady, joint evolution of public corporations and securities markets" (p. 2). In other words, not to worry. The shenanigans of Ivan Boesky and Michael Milken are simply modern expressions of a healthy, free market that should not be encumbered by government regulation that might demand "corporate social responsibility," which would sacrifice "shareholder welfare." Werner and Smith conclude that as "long as profit maximization is one of society's highest values, public corporations and securities markets appear to be the best methods for achieving it," and "nothing should be allowed to upset the internal governance structures that have, along with unique capabilities for mobilizing resources and adapting to change, so well served the corporate system in its steady expansion" (p. 156).

This book unfortunately attempts to put history to work to justify private greed and celebrates the "New York securities market," which by 1840 "had realized its ultimate native genius: its capacity for unrestrained speculation" (p. 43).

JAMES ROGER SHARP
Syracuse University

LARRY LANKTON. *Cradle to Grave: Life, Work, and Death at the Lake Superior Copper Mines*. New York: Oxford University Press. 1991. Pp. xi, 319. \$39.95.

This eloquent volume on the development and decay of copper mining on Michigan's Upper Peninsula stands as another in a distinguished line of studies concerned with the hard realities of extracting metals and fuels from a forbidding underworld. Like Anthony Wallace's *St. Clair: A Nineteenth-Century Coal Town's Experience with Disaster-Prone Industry* (1987) and Donald Miller and Richard Sharpless's *The Kingdom of Coal: Work, Enterprise, and Ethnic Communities in Mine Fields* (1985), Larry Lankton's analysis of Keweenaw mining is informed by close familiarity with the history of the relevant technologies, dynamics of laboring communities, and relations between workers and companies. In contrast to eastern coal or western metal operations, however, Michigan copper mining featured a durable paternalism practiced by leading corporations, placid labor relations, and, for several generations, relatively stable communities, as Lankton documents concisely.

The Upper Peninsula's copper resources were first appropriated by Native Americans who harvested geologically rare lumps of pure metal. Colonial and early republican efforts at mining by incomers routinely failed until shortly before the Civil War, when new techniques inaugurated seventy years of increasing productivity at three extensive copper-bearing ranges. The costs and risks of these capital-intensive enterprises favored the prospects of large firms, among which Lankton profiles the Quincy, Copper Range, and Calumet and Hecla companies in greatest detail. The immense scale of their "lodes," the region's harsh winters, and the need to attract and retain thousands of skilled workers led such firms to the early establishment of elaborate, if uneven, corporate-welfare programs, administered with long-term consideration for profitability, a dramatic contrast to the short-term exploitiveness Wallace underscored for anthracite coal.

The Michigan ranges peaked in importance and dividends paid between 1890 and 1920. A bitter strike in 1913-14, occasioned by efforts to install one-man rock drills that would cut labor costs yet isolate miners and increase their risk of injury, signaled but did not cause the onset of decline. Lankton carefully avoids making this conflict the centerpiece of his narrative, showing convincingly that it was an incident within rather than the essence of the downward slide. Michigan firms, which by 1900 had taken the richest and most easily reached copper-bearing rock, simply could not meet the cost-profiles of western giants such as Anaconda and Phelps-Dodge, much less those of rising international sources. World War I brought a brief boom, but contraction thereafter was inexorable.

Lankton's study is thematic, taking the reader from the heart of the mines to the rock-crushers on the

surface and the ethnic residential clusters that surrounded them. It includes admirably clear descriptions of complex technologies and their evolution; has powerful passages on accidents, safety practices, and usually humane company supports for the maimed or widowed; and it is written in a vigorous and engaging style. Lankton is especially elegant in depicting the situational complexities of introducing technical innovations, and his work should stimulate historians to consider fruitful comparisons with other aspects of mining, both in the United States and internationally. Although a fuller account of central office practices might be desired, this study is otherwise a model of interdisciplinary research in business, labor, ethnic, and community history and the history of technology.

PHILIP SCRANTON
Rutgers University,
Camden

MYRON B. KUROPAS. *The Ukrainian Americans: Roots and Aspirations, 1884–1954*. Toronto: University of Toronto Press. 1991. Pp. xxvii, 534. \$50.00.

In common with many Slavic immigrants who migrated to the United States at the end of the nineteenth century, persons from the Ukrainian territories governed by Russia and the Austro-Hungarian empire did not possess well-developed ethnic identities. In America, however, immigrants from these territories formed identities as Carpatho-Rusyns, Russians, or Ukrainians. Myron B. Kuropas focuses on the development of the Ukrainian-American identity. Arguing that the United States was "a school for . . . ethnonational development" (p. 14), Kuropas holds that the Ukrainian-American identity evolved as a response to influences and events in both the United States and Europe. This "metamorphosis . . . from a Ruthenian religiocultural identity to a Ukrainian ethnonational identity" (p. 74) occurred progressively in three chronological stages. In the pre-World War I era, religious and ethnic struggles forged a Ukrainian-American identity. During the interwar period, religious differences, together with differing political ideologies regarding the homeland, tempered that identity. Finally, the 1940s witnessed the emergence "of a unique and peculiarly Ukrainian-American ethnonational hybrid" (pp. xix–xx), but the inability to achieve cross-generational cultural maintenance seemingly doomed the Ukrainian-American community. According to Kuropas, the Ukrainian immigration of the 1950s stopped this decline and reinvigorated the Ukrainian-American community.

Kuropas attempts to describe the interplay of internal and external pressures that influenced Ukrainian-American ethnicity. He chronicles intragroup rivalries, religious disputes, and political divisions that spawned and nurtured a Ukrainian-American identity. Focusing on external forces, he reviews the

debates among Ukrainian Americans regarding the fate of Ukrainian lands. From the outset, Kuropas makes it clear that organizations and "organizational life" are the foci of his work. Organizations, he contends, provide a means to analyze ethnic consciousness because they "are reflections of the sentiments and values of their members" (p. xx). Kuropas remains loyal to this approach. Despite thematic titles, chapters are typically subdivided by organizations. Actually the volume comes closer to being an anthology of jubilee histories than an interpretive history. Much of the narrative comprises battles among organizations or groups as well as among lay and clerical leadership. This segmented format, unfortunately, creates redundancies and obscures analytical points. Moreover, the work is laden with a staggering number of abbreviations, inordinately lengthy quotations, and perfunctorily itemized material.

The broad subtitle of the book is misleading. The roots and aspirations analyzed are narrowly limited to the emergence of a Ukrainian-American ethnonational identity and Ukrainian nationalism. Persons looking for information about other, possibly more mundane, aspirations of Ukrainian immigrants will not find them in this work. Moreover, despite its organizational focus, this volume is not dispassionate history; it is written from a decidedly nationalist perspective. By referring simply to "Ukraine" without the definite article "the," Kuropas confers a sovereignty on the region. Celebrating expressions of Ukrainian nationalism and Ukrainian-American ethnonationalism, the author expends tremendous energy countering criticisms leveled against Ukrainian-American organizations, especially in the 1930s and 1940s. He labors to explain and, at times, justify or rationalize some activities of Ukrainians in the United States and Europe.

Such weaknesses notwithstanding, scholars will find this work a useful reference. In encyclopedic fashion, it contains the history of an array of Ukrainian-American organizations. The extensive bibliography is invaluable, the starting point for future analytical histories of Ukrainian Americans.

JUNE GRANATIR ALEXANDER
University of Cincinnati

LEWIS L. GOULD. *The Presidency of Theodore Roosevelt*. (American Presidency Series.) Lawrence: University Press of Kansas. 1991. Pp. xii, 355. \$29.95.

This book admirably fulfills the purpose set for it and for the series in which it appears. Lewis L. Gould's volume presents a clear, balanced, insightful account of Theodore Roosevelt's seven and a half years in the White House. It makes a welcome contribution because, as the author observes, despite a plethora of attention to Roosevelt's pre and post-presidential periods, this is the first account devoted exclusively to his presidency. Gould gives the right amount of

attention and sound analysis to nearly all aspects of Roosevelt's policies and actions, including diplomacy, conservation, economic reform, and party politics. He likewise conveys a good sense of Roosevelt as an administrator who recruited able lieutenants and infused a special zest into service under him. Gould also nicely depicts the personalities and doings of both the president's supporters and his adversaries, particularly Republican leaders on Capitol Hill. Withal, the book establishes itself as the best treatment yet done of its subject.

The book has two additional major strengths. First, although Gould adopts a properly respectful and usually positive stance toward his main character, he keeps a critical distance. He finds Roosevelt deficient in several aspects, such as sometimes unnecessarily antagonizing the congressional satraps, too readily trusting people who agreed with him, and turning deaf ears to disagreement. He also notes that Roosevelt enjoyed much luck in not having to deal with foreign threats or bruising internal conflicts. The book's other additional strength lies in its perspective on the evolution of the presidency. Gould correctly finds that Roosevelt built on innovations of his predecessors, especially William McKinley, in a long-running process of strengthening the office. Still, Roosevelt pushed that process dramatically forward. Gould concludes, "He dramatized and personalized the modern form of his office and made it a living reality for Americans of his day and for the generations that followed" (p. 301).

Fine though this book is, however, it has flaws. Three are particularly striking. First, in light of the controversy of court decisions at that time and recurrently since then, Gould makes surprisingly scant mention of Roosevelt's views on the role of the judiciary and constitutional interpretation. That omission probably stems from a second, larger flaw. This book deals almost entirely with what Roosevelt did as president, rather than with what he thought. With some, perhaps most, presidents, such an emphasis can suffice—but not with this Roosevelt. He remains one of the only two genuine intellectuals to occupy the White House in the last century and longer (the other was Woodrow Wilson). Those who want to observe what Lewis Einstein once called Roosevelt's "mind in action" must look elsewhere.

Finally, this book leaves an unsettling question about its subject, the Roosevelt presidency. How important was it? Gould concludes that, given the absence of great crises and political upheavals during Roosevelt's reign, he stands only in the second rank of presidents, in the category of "near great." May that not be the reason why previous works about him have concentrated more on the years before and after the White House? May it not be that the most interesting, revealing, and formative time in his life came earlier and the most significant time came later? Theodore Roosevelt was no George Washington or Abraham Lincoln, and he was the first to recognize and regret

the difference. Roosevelt's sense of missed greatness shaped his thoughts and deeds in his post-presidential decade, when titanic events did transpire at home and abroad. This fine, balanced book treats its subject well, but the best—not the happiest, but the truly significant time for its main character, his nation, and the world—was yet to come.

JOHN MILTON COOPER, JR.
University of Wisconsin,
Madison

JOHN E. O'CONNOR. *Image as Artifact: The Historical Analysis of Film and Television*. (American Historical Association Institutional Services Program.) Malabar, Fla.: Robert E. Krieger. 1990. Pp. xi, 344. \$46.50.

After several decades, the visual communications media are now securely within the walls of the historical profession. The evidence provided by moving images is increasingly essential rather than merely trivial or banal, and few would quarrel with John E. O'Connor's contention in this useful collection that film and television are "shapers of historical consciousness" (p. 2). History survey texts now give more than a cursory look at the media, there are film review sections in some history journals, and the American Historical Association is on board. This work was developed as part of the Association's Institutional Services Program.

O'Connor and his contributors have presented, for the most part, a no-nonsense evaluation of recent historical works using film and television. Most of the essays in this collection are both didactic and self-critical. The authors hope historians may learn to use these media more critically and train students in the same light. This "effective guide" or "catalog" commendably aims at adding "visual literacy" (p. x) to historical scholarship.

The collection analytically subdivides into the areas of content, production, and reception of what is seen. In the first stage of inquiry, information on all these categories should be gathered. Second, the analysis focuses on four interrelated but relatively independent aspects: the image as historical representation, the image as evidence for sociocultural history, the image as historical actuality, and the history of the image itself, both as industry and as art.

The defensive tone that has been so strident among historians interested in film is muted somewhat here, but still persists. O'Connor claims historians must undertake serious consideration of the moving image as historical representation or orientation unless they wish only to write for each other. The efforts in the collection are offered, with beguiling immodesty, to "assist in rehabilitating the public image of the professional scholar and teacher" (p. 3). This is the historical version of the First Church of What's Happening Now, made seemingly more urgent by the global saturation of television.

None of the authors really marches in the editor's footsteps in this respect, and most are content to make the point, as Daniel Leab does, that the evidentiary problems inherent in the study of film and television are present to a certain extent in traditional historical analysis as well. O'Connor himself suggests that part of the reason to incorporate the visual media in the classroom may be the simple and understandable desire to interest a wider audience.

The major problem with the collection by far is in the area of historical theory. Hollywood director John Ford, for example, described himself as someone who made Westerns, but scholars have trouble leaving such statements well enough alone. In the hands of a relentless theoretician, approaching the evidence model first, Ford might become "a historical user of a preexistent signifying practice" (p. 252). What has occurred, apparently, is that the European (largely French) penchant for theorizing about the media is confronting the carelessly pragmatic and relentlessly capitalistic American film industry, by far the most powerful on earth. Insistence on some kind of method or some insightful paradigm dominates a few of these contributions, restlessly searching for some kind of mold into which to force the Hollywood product.

The paradigmatic possibilities are plentiful, and Janet Staiger, in a helpful run-through of the arcanum, lists a few: poststructuralism, Lacanian psychoanalysis, Althusserian Marxism, and semiology. "Re-analysis" is now possible, says Staiger, because the "explicit notion that histories imply explanatory models [has become] widespread" (p. 254).

Historians, of course, are not the only scholars mining these lodes, and much of the "modeling" has come from communications studies, film studies, and popular culture approaches. But, of the eleven contributors, only Robert Sklar bothers to bow in the direction of common sense (and, one might add, to ad hoc rather than paradigmatic research), although Staiger delivers a backhand in which such an approach becomes "hegemonic notions about human behavior" (p. 262). Staiger offers an unintentionally hilarious example in which she, David Bordwell, and Kristin Thompson propose a "new history and textual analysis of the classical product" using "a revision of Russian formalism, Czech structuralism, and semiotics through a modified Marxism." They found that Hollywood was not driven by "a teleology of realism or an ideology of illusionism . . . but rather by the (economically advantageous) desire to produce narratives" (p. 257). In other words, Ford and the others were in it for the bucks.

"How better to begin to judge the success of a film," asks O'Connor, "than to consider it in light of the specific goals set down by the producers in the planning of the project" (p. 18). Indeed. It is not reductionism to freely admit the overriding profit motive in the Hollywood or commercial television product, and work from there. The question is not, as Staiger

implies (p. 267), "which [modular] representation is the more adequate one?" It is, instead, how can the available and pertinent evidence best be gathered, shaped, and presented?

These are the concerns of more traditional historians as well. The pompous claptrap here, fortunately, is in a minor key. While O'Connor correctly concludes that "historians concerned with the moving image cannot afford to turn their backs on film theory" (p. 324), neither can they embrace theory wholeheartedly (a most promiscuous embrace at this juncture) to the exclusion of the more cautious but more useful concerns of the historian.

MICHAEL T. ISENBERG

United States Naval Academy

MIRIAM HANSEN. *Babel and Babylon: Spectatorship in American Silent Film*. Cambridge: Harvard University Press. 1991. Pp. x, 377.

This work is a bold and strikingly original exploration of spectatorship in American silent film. Miriam Hansen has grounded her work in a vast body of historical scholarship and film criticism, including the latest historical and theoretical debates. She synthesizes this information with remarkable skill and moves the discussion forward by demonstrating how class and gender informed spectatorship in the "alternative public sphere" where ethnically, sexually, and socially diverse audiences consumed cinematic culture.

Hansen argues that the silent film era from the 1890s through the end of the 1920s marks a transformation of the relationship between public and private domains in popular entertainment. This period saw the homosocial traditions of vaudeville, spectator sports, and the saloon eroded by the birth of a cinema in which female spectatorship and female desire left their imprint on an emerging culture of consumption. Hansen radically alters the concept of spectatorship as it had been articulated in psychoanalysis and semiotics by grounding that concept in the public sphere by insisting on the public dimension of cinematic reception. Drawing heavily on the Frankfurt School's debates over Jürgen Habermas's publication of *The Structural Transformation of the Public Sphere* (1962), she demonstrates how the asymmetries of gender complicated the disintegration of the bourgeois public sphere so that the increasing perception of women as subjects of consumption coincided with their empowerment as cinematic consumers who constructed a feminine subculture as powerful as that of the nineteenth-century cult of True Womanhood, yet one far more subversive of the patriarchal control of sexual desire.

Hansen explicates her thesis that American silent film offered its viewers both Babel, the myth of a universalizing, ideological idiom, and Babylon, a polymorphously perverse world of fantasy that al-

lowed for spectatorial freedom, in terms of two exemplary cinematic moments. She describes D. W. Griffith's *Intolerance* as a hieroglyphic film that simultaneously conveyed a vision of cinema as a universal language and employed a protostructuralist narration. It integrated narratives from four distinctive historical periods that asked viewers to engage in an interactive construction of the film's meaning rather than to adopt spectatorial positions determined by the film's *auteur*. Hansen indicates how this film by a patrician southerner reveals a crisis in femininity in which the ideal of a separate female sphere enshrined in the film's emblem, the Woman Who Rocks the Cradle, has been eroded by the interpenetration of public and private spheres. Hansen also examines the cult of Rudolph Valentino as a star, noting how his alien ethnicity and deviant sexuality informed a discourse on female sexuality.

Although Hansen emphasizes the degree to which flirtation with ethnic otherness enhanced the spectatorial freedom of women, she tends to neglect the impact of acculturation on the shifting desires of ethnic women. She begins her book with an examination of the Corbett-Fitzsimmons fight (1897) and notes the unusually high level of female attendance. For Jewish women, however, an interest in pugilism would be transgressive not only in terms of feminine modesty but also in terms of Jewish identity, since physical prowess was associated with menial labor and the gentile world and not with the realm of ideas occupied by the ideal of the Jewish scholar. Similarly, for a feminine culture that at the turn of the century could embrace the Yiddish actor Boris Thomash-evsky, described by Hutchins Hapgood as fat and phlegmatic, as its romantic ideal to turn to the dashing and deviant Valentino represents a remarkable shift in the way in which Jewish women constructed their sexuality, one that would make Valentino an agent of their acculturation rather than an invitation to explore an alien identity. And one should not assume that only women profited from the possibilities of alien and deviant forms of sexuality on screen, for surely the films of Cecil B. De Mille and Erich von Stroheim suggested that a leavening of forbidden European pleasures may have redeemed the banality of American marriage for both spouses alike. Despite these minor caveats, Hansen has produced a work that has revolutionized the concept of spectatorship in American silent film and that will be an essential tool for historians and film scholars alike.

LESLIE FISHBEIN
Rutgers University,
New Brunswick

MARTIN E. MARTY. *Modern American Religion*. Volume 2, *The Noise of Conflict, 1919–1941*. Chicago: University of Chicago Press. 1991. Pp. xiv, 464. \$27.95.

Martin E. Marty has now completed the second installment of a projected four-volume narrative history of modern American religion. His overall scope corresponds roughly with what Protestant liberals around 1920 optimistically designated as "the Christian Century"; a designation appropriately, if somewhat ironically, enshrined as the title of the journal of opinion rehabilitated by Charles Clayton Morrison, a participant-observer in liberal American Protestantism whom Marty invokes frequently, and of which Marty himself now serves as an editor.

Marty makes clear in his introductory remarks that he is not trying to write a comprehensive history of American religion in the century now approaching its close but rather is focusing on "public religion." In this emphasis on the public realm—the visible interaction of secular and religious ideas, movements, and institutions—he clearly echoes the concerns of the figure who dominates this work, Reinhold Niebuhr. In his *Irony of American History* (1952) and other works, recently reappraised in Richard Fox's biography, Niebuhr introduced the notion of "irony" into the theological vocabulary, and he stands out clearly as Marty's hero. (The subtitle of the preceding volume in this series is "The Irony of It All.")

This volume proceeds both chronologically and dialectically. The theological drama centers on Protestant liberalism or modernism, which generates its antithesis in fundamentalism. The clash becomes most painfully and publicly visible in the Scopes trial of 1925. Neo-orthodoxy, best exemplified in the work of Niebuhr and his brother, H. Richard Niebuhr, is the resultant synthesis. Complementing the intellectual drama is the controversy over the economic disaster of the Great Depression. Here the most memorable protagonists are Roman Catholics, with Father Charles Coughlin as evil angel, and Dorothy Day as the corresponding angel of light.

Although Marty disclaims the role of social historian, a theme that helps unify this work is eminently social: the struggle of "original stock" American Protestants to maintain their hegemony over national life. The question of "Americanism" has obvious reverberations in the present and is here evoked both in nativist reactions such as the resurgent Klan of the 1920s, as well as in the dramas of adaptation and resistance played out in groups ranging from the vast Roman Catholic church to the tiny Jehovah's Witnesses. Without attempting exhaustiveness, Marty skillfully cites representative issues in these groups to examine the dilemma of minorities in a complex and rapidly changing society in which the traditional majority is splintering and running scared.

Although Marty does not introduce much startlingly new data, he makes a major contribution to our understanding of American religion and culture thorough his creation of a readable, balanced, and perceptive narrative that eschews exhaustive detail in favor of selected events, major or obscure, revealing broader patterns of significance. In an age of com-

partmentalized scholarship and fragmented theology, he implicitly makes the case for a steady and holistic vision that brings old lessons to bear on newly configured but curiously familiar issues. Marty thus continues to act simultaneously as detached historian and engaged participant with a concern for both the larger meaning and the common good. Bravo!

PETER W. WILLIAMS
Miami University
Oxford, Ohio

BRADLEY J. LONGFIELD. *The Presbyterian Controversy: Fundamentalists, Modernists, and Moderates*. (Religion in America.) New York: Oxford University Press. 1991. Pp. 333. \$34.50.

This study adds to the list of good books published in recent years on the fundamentalist-modernist controversy of the 1920s. Bradley J. Longfield describes one of the battles in the conflict, the struggle for control of the northern Presbyterian church. That fight began with a conservative-led effort to expel Harry Emerson Fosdick from the church in 1922 and ended with the defrocking of fundamentalist leader J. Gresham Machen in 1936. More than any other episode, the Presbyterian story lays to rest the simplistic notion that the fundamentalist-modernist controversy was a fight between educated liberals and conservative rubes and ignoramuses. Although anti-intellectualism was characteristic of some groups of fundamentalism, "fundamentalists and modernists within the Presbyterian community were divided not so much because they valued the intellect differently but because they subscribed to variant philosophical presuppositions" (p. 223).

The general outlines of the Presbyterian controversy are well known. Up until 1925 Presbyterian conservatives won a series of close victories, including the election of Clarence E. Macartney as denominational moderator in 1924. In 1925, however, Charles R. Erdman, a centrist Princeton professor, was elected moderator. Erdman and other Presbyterian moderates were orthodox Presbyterians, but they believed that the church should embrace "theological pluralism." They rejected and attacked the separatist notions of hard-line fundamentalists such as Machen. As moderate control of the denomination gained strength in the late 1920s, conservatives lost control of Princeton Seminary, long the stronghold of Machen. In the 1930s Machen lost a fight with Robert E. Speer, the head of the church's Board of Foreign Missions, a defeat that caused him to leave the church.

Longfield weaves into this denominational story the intellectual biographies of six major actors—Machen, William Jennings Bryan, Henry Sloane Coffin, Macartney, Erdman, and Speer. In some ways, this strategy works. The detailed biographical sections demonstrate the diversity within the church and sustain

the author's basic thesis that "theological, ecclesiological, philosophical, and cultural [factors] contributed to the advent and final outcome of the Presbyterian conflict" (p. 218).

From a literary point of view the technique is less successful. The biographical inserts are disruptive and unnecessarily detailed, and they are based almost entirely on secondary works. The author footnotes every trivial detail—from the fact that in 1877 New York City was "the modern capital of the nation" (p. 80) to the news that Erdman was "a congenial-looking man of moderate height" (p. 138).

In spite of these remnants from graduate school, this is a good book. It takes all sides in the controversy seriously and treats them fairly. It is easy to cast Machen as the villain and to offer sainthood prematurely to the enlightened liberals. Longfield escapes that trap partly because he is troubled by the ailing Presbyterian church that the liberals bequeathed to the late twentieth century. "Without clear theological boundaries," he judges, "the church, in the years ahead, would find it more and more difficult to maintain an identity separate from the culture and offer a unique message and vision to the world it sought to serve" (p. 230). Longfield's book is not a defense of fundamentalism, but neither is it a brief for liberalism. It will reinforce the conclusion that the fundamentalist-modernist controversy was not simply a fight of good against evil and enlightenment versus ignorance.

DAVID EDWIN HARRELL, JR.
Auburn University

DAVID J. ROTHMAN. *Strangers at the Bedside: A History of How Law and Bioethics Transformed Medical Decision Making*. New York: Basic Books. 1991. Pp. xi, 303. \$24.95.

During the past couple of decades, David J. Rothman has acquired a well-earned reputation as one of the historical profession's most passionate critics of American medicine. In books such as *The Discovery of the Asylum* (1971) and *The Willowbrook Wars* (1984) he sought to expose the punitive and abusive treatment of patients in institutions for the mentally ill and retarded. In this latest work he explores the moral backlash created by revelations in the 1960s and 1970s of nontherapeutic experimentation on powerless persons and of ethically questionable practices related to babies born with birth defects. As a result, lawyers, bioethicists, and other nonphysicians began crowding around the bedside, instructing physicians what to do and effectively ending their monopoly over medical decision making. But the title of this book holds a dual meaning. Almost in passing Rothman also explains how the decline of the house-calling family doctor and the rise of the hospital-based specialist turned even the physician at the bedside into a stranger.

Rothman spends the first quarter of his book sketching the background to the bioethical revolution. He skims over the history of human experimentation before World War II and devotes a chapter each to wartime and postwar research, when, he claims, laboratory and clinical breakthroughs gave researchers license to experiment on human subjects almost at will. The heart of the book focuses on the emergence of "new rules" for the laboratory and the bedside during the decade between 1966 and 1976. The transition began when a Harvard physician, Henry Beecher, published a scathing indictment of experimental practices, showing, in Rothman's words, that "The bedrock principle of medical ethics—that the physician acted only to promote the well-being of the patient—did not hold in the laboratory" (p. 89). The movement of outsiders into the medical arena grew with the competition for organ transplants and the concern for defective newborns. The decade ended with the New Jersey Supreme Court ruling that the parents of the permanently comatose Karen Ann Quinlan could remove her from the respirator against medical advice, a decision that witnessed the movement of medical decision making into "the public domain" and showed "that a profession that had once ruled was now being ruled" (p. 222). With these developments lawyers and bioethicists who had "first superintended the work of physicians in the laboratory" now moved into "the infant nursery and adult intensive care unit" (p. 190).

In this well-written and sometimes riveting book, Rothman illuminates a major transformation in American medicine. Yet at times his tone is too judgmental and his coverage too episodic for my tastes. He seems not to recognize, for example, that "the boundaries of acceptable ethics" (p. 27) have changed with the times. And, relying on a spotty use of largely secondary sources, he sometimes misreads nineteenth and early twentieth-century events, overestimating concern with the welfare of experimental subjects and underestimating the frequency of human experimentation. In surveying practices before World War II, he inexplicably fails to mention the notorious Tuskegee syphilis experiment that began in the early 1930s; Tuskegee does not enter Rothman's story until the early 1970s, when it became newsworthy. Nevertheless, this is an important book that deserves wide readership.

RONALD L. NUMBERS
University of Wisconsin,
Madison

BRUCE KUKLICK. *To Every Thing a Season: Shibe Park and Urban Philadelphia, 1909–1976*. Princeton: Princeton University Press. 1991. Pp. xii, 237. \$19.95.

Noted intellectual historian Bruce Kuklick examines the history of Philadelphia's Shibe Park, the first modern major-league park (1909), and its relation-

ship to the city because of his interest in the affectional importance of public places. He wants to understand the feelings people had about the edifice, and how that influenced "the way sport is instrumental in ordinary people's construction of a meaningful past for themselves" (p. 200).

In the first part of the book (1909–32), Kuklick examines the establishment of the Philadelphia Athletics and their on-field achievements, emphasizing the roles of founder Ben Shibe and manager-owner Connie Mack, the building of the new grounds, and economic and other relationships between the North City area and Shibe Park. The second part of the book (1929–54) covers the period from the Great Depression through the migration of the club to Kansas City, examining the financial operations of the underfinanced Mack family, particularly the connections of urban politics to the business, and the renting of the park for such public functions as Phillies baseball games (1938), Eagles football games (1940), boxing matches, and political and religious rallies. In the final section (1953–76) Kuklick superbly analyzes the demise of Shibe Park, devoting considerable attention to the race question. The city's racist baseball heritage discouraged black attendance and disinclined integration of the team. As long as the neighborhood was white, there was a strong sense of community identification with Shibe Park because it provided jobs to residents and the locality housed many ballplayers. When the surrounding neighborhood declined in the 1950s, however, and turned from white to black, the ballpark became a pawn in struggles between sports entrepreneurs, politicians, developers, and urban planners. The grounds became simply a parcel of devaluated real estate that had lost most of the value accrued as a field of dreams.

Kuklick's epilogue is a first-rate analysis of the social functions of baseball's cyclical history, in which memories of great achievements are handed down from generation to generation. He argues that the sport taught people to respect excellence and to experience uncommon deeds in physical contests that were characterized by grace under pressure, endurance, valor, and victory, and dignity in defeat. Baseball at the old ballpark provided a means of endowing fans with a shared identity and a historical consciousness. The ballpark was home to "a semisacred collection of handed-down stories, and recollections about sports, politics, and mass entertainment." Yet the experience transcended the physical presence of Shibe Park since the memories remained alive even after the ballpark was torn down because "the precious places of baseball were really in people's hearts and minds" (p. 191). Kuklick successfully argues that the ballpark was home to stories and memories, but I do not think he makes a case for politics or mass culture.

Kuklick uses a wide variety of sources, including local newspapers, court cases, business records, fire

insurance and land-use maps, and interviews, although he did not consult the National Baseball Library. Readers may find his endnotes difficult to use. Kuklick is probably too sympathetic to Connie Mack, and his narrative of early baseball history is cursory. I found certain hypotheses to be dubious, such as the questioning of the integrity of the 1914 World Series. Despite these criticisms, I learned quite a bit from this book.

STEVEN A. RIESS
Northeastern Illinois University

KENNETH W. GOINGS. *"The NAACP Comes of Age": The Defeat of Judge John J. Parker. (Blacks in the Diaspora.)* Bloomington: Indiana University Press. 1990. Pp. xii. 125. \$17.95.

Within two decades after its founding in 1910, the National Association for the Advancement of Colored People (NAACP) scored a small but significant number of legal victories in the quest for black equality. Beginning with the *Guinn* case (1915) that abrogated the Oklahoma grandfather clause, the protest organization served notice of its determination to wage war on a system of Jim Crow that severely limited black rights. Despite some success, the NAACP's future remained in considerable doubt, especially in many parts of the South where whites often used physical intimidation and economic reprisals against those who joined the black group.

In this study, Kenneth W. Goings has examined how the NAACP's opposition to the appointment of Judge John J. Parker to the United States Supreme Court contributed to the growth of the organization and helped to politicize the black community. A nominee of President Herbert Hoover in 1930, Judge Parker, a North Carolinian, had often displayed hostility toward labor, and he had spoken out against black political rights. Although the NAACP had adopted essentially a legal approach to achieve its ends, it decided to carry out an intense lobbying campaign to defeat the Parker candidacy in the Senate. While labor and some other groups also opposed the nominee, Goings argues that the issue of race was a crucial consideration in Parker's demise. That contention, of course, has unquestioned merit, but the very fact that some southern senators voted against the judge points up the significance of other issues.

The various constituencies that opposed Parker later became important in the New Deal coalition. In analyzing the relationship of blacks to that coalition, Goings has made his most notable contribution. He rightly contends that the Parker nomination and the efforts of blacks and their friends to defeat those senators who supported the judge were "a major step in helping to move the black electorate from the Republican column into the Democratic column" (p. 92). While Goings stands on solid ground here, he

could have significantly strengthened his case by a comparative analysis of the black vote in select black precincts across the country for the period before 1930 and for the 1932 election. Goings's contention that the campaign against pro-Parker senators contributed to the politicizing process of the black community is more strongly documented. He also correctly observes that the Parker episode made the NAACP, a nonpartisan organization, the most important political voice of the black community.

This book expands our knowledge of the NAACP and the politics of black America. The author has built his study on a solid body of primary sources while relying heavily on previous studies, especially Richard Watson's article, "The Defeat of Judge Parker: A Study in Pressure Groups and Politics," *Mississippi Valley Historical Review* 50 [1963], 213-34. Although this book possesses considerable merit, the work contains some distracting and misleading statements. For example, the author confuses the Gospel of Wealth with the Social Gospel (p. 2), and the election of 1920 with that of 1928 (p. 44). Moreover, to lump Marcus Garvey and A. Philip Randolph together as "black nationalists" (p. 12) obscures the great distance between the two men, especially Randolph's disdain for Garvey's back-to-Africa scheme. Criticisms aside, Goings has broadened our understanding of an important topic, and his book deserves a reading.

JIMMIE LEWIS FRANKLIN
Vanderbilt University

NICHOLAS LEMANN. *The Promised Land: The Great Black Migration and How It Changed America.* New York: Alfred A. Knopf. 1991. Pp. 410. \$24.95.

This compelling book tells two interrelated stories: one, the story of the migration between the early 1940s and the late 1960s of more than five million black Americans from the cotton plantations and small towns of the rural South to the ghettos of the urban North; the other, the story of the evolution of federal social policy in the 1960s and early 1970s in response to this extraordinary demographic shift. Combining hundreds of interviews with exhaustive research in archival sources, Nicholas Lemann, national correspondent for *The Atlantic*, guides the reader back and forth between three places that anchor his skillful narrative: Clarksdale, Mississippi, where the mechanization of cotton picking pushed blacks out of the Delta; the south side of Chicago, a promised land of economic opportunity and human dignity, where the migrants' dreams of a better life collided with the realities of slum housing, joblessness, dysfunctional families, welfare dependency, and crime; and Washington, D.C., where policy makers struggled with a northern racial crisis that they neither anticipated nor fully understood.

Along the way, Lemann paints a richly detailed

human portrait of individual families who exemplify in their own odysseys the broader social forces of migration, ghetto formation, and urban decay that have so powerfully shaped the landscape of the late-twentieth-century urban North. By turns ambitious and devoid of hope, triumphant and tragic, these men and women are by far the most compelling figures in Lemann's complex tapestry. Less humanly engaging, but no less important to Lemann's story, are the policy makers in the White House and the federal agencies whose personal ambitions and political agendas produced initiatives (community action and community development chief among them) that at once ameliorated and intensified the complex problems of the ghettos.

For scholars, this book offers the most complete account we have of migration and ghetto formation in this period, along with a contribution to the theoretical debate over the problem of the underclass. (Lemann links the social pathology of the urban ghettos of the North to the southern rural sharecropping culture from which they were peopled.) For general readers as well as policy makers, the book offers the hopeful message that the underclass is not beyond redemption, that the problems of the ghetto can be solved through intensive, carefully targeted federal intervention. The lesson of the 1960s, Lemann argues, is not that federal programs do not work, but that some do work—programs in family planning, prenatal and infant health care, early childhood education, and job training, for example, programs that have had clear, measurable effects on the incidence of teenage pregnancy and infant mortality, on school achievement levels, on graduation rates, and on employment rates. He calls "an ambitious wave of new programs of this kind . . . the best chance we have to make a real difference in the ghettos" (p. 350). Lemann dismisses the notion that such an effort is beyond our conceptual grasp or beyond our means; "the real impediment," he says, lies in a pervasive "weakness of spirit" (p. 351). His book stands, ultimately, as a stirring exhortation to Americans to summon the will to address "the most significant remaining piece of unfinished business in our country's long struggle to overcome its original sin of slavery" (p. 344).

NANCY J. WEISS
Princeton University

GARY MUCCIARONI. *The Political Failure of Employment Policy, 1945–1982*. (Pitt Series in Policy and Institutional Studies.) Pittsburgh: University of Pittsburgh Press. 1990. Pp. xi, 317. \$34.95.

The federal government spent nearly \$77 billion between 1963 and 1982 to provide jobs and training for workers. The effort included the Manpower Developmental and Training Act of 1962, the Great Society's poverty program, the Emergency Employ-

ment Act of 1971, the massive Comprehensive Employment and Training Act (CETA) of the mid-1970s, and the Job Training Partnership Act of 1982. Additional monies supported unemployment insurance and the public employment service. Gary Mucciaroni, a political scientist, maintains that overall it was a dubious bargain. He argues that U.S. employment policy in the years after World War II enjoyed sporadic success in reducing joblessness but resulted chiefly in disappointment and political failure. The failure took several forms: the assignment of employment policy to the peripheral role of serving the social-welfare needs of the unemployed instead of to the central task of national economic management; the inability to sustain a consensus regarding which social-welfare objectives to emphasize; and, finally, the actual poor performance of the programs that resulted from such broader confusion, a performance that lent credence to conservative accusations that government was not the solution but rather was itself a large part of the employment problem.

Mucciaroni sets out not merely to describe the failure of employment policy but also to explain it. He examines both the "employment policy subsystem," which addresses narrow, nuts-and-bolts employment issues, and the more amorphous national political economy, which addresses broader questions of economic means and ends. His analysis ultimately focuses on the influence of ideas, institutions, and interests in the shaping of policy. Each of these factors "played a dominant role in shaping a different aspect of employment policy" (p. 257). Ideas were crucial in determining the overall direction of policy, institutions in guiding the actual administration of programs, and conflicting interests in establishing the level and distribution of benefits. The author also stresses a fourth factor: what political scientist Hugh Heclo called "political learning"—the process by which public officials, experts, and interest groups respond creatively to past performance and to present problems and opportunities. Political learning influenced virtually all aspects of employment policy and accounted for what Mucciaroni views as that policy's highly contingent nature. To underscore this contingency and to illustrate the range of possible policy outcomes, the author includes a chapter contrasting the American and Swedish approaches to the issue.

This study is not the book most historians would have written. There is no strong narrative line, little reliance on primary sources, and scant effort to bring individual historical actors to life. But these omissions are more than offset by the power of Mucciaroni's analysis. He seeks to explain an important phenomenon in American public life and does so clearly and compellingly. The volume will be standard reading for anyone interested in postwar employment issues. But one would hope for a broader audience as well. All historians interested in public policy will find this study instructive for its clear-headed way of looking

at the basic nature of policy formation and administration. In this case, political science has much to teach us.

ROBERT M. COLLINS
*University of Missouri,
Columbia*

JOHN ARTHUR MAYNARD. *Venice West: The Beat Generation in Southern California*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xi, 242. \$22.95.

This work by John Arthur Maynard is not a typical academic book, and that is part of its charm. For example, it presents nine chapters but no chapter titles. In place of footnotes there are conversational "notes on sources." There is a descriptive subtitle but it is far too narrow; Maynard consistently tells us about the Beat generation in America and not just in Southern California.

The book's organization is sometimes chaotic, but the strong narrative and perceptive analysis overcome the disarray. I believe it will be a starting point for most future Beat research. The book centers on two less-famous Beat figures, Lawrence Lipton and Stuart Perkoff. During the 1950s, this pair settled in Venice, California, a ragged suburb of Los Angeles. Aided by national publicity, Lipton and Perkoff helped make Venice a center of Beat culture in the late 1950s and early 1960s.

Born in 1897, Lipton was an older Beat who participated in every radical American cultural movement until his death in 1975. He yearned to be the classic Beat writer. He ended up, however, a Beat publicist and cheerleader. His feel for what magazines and publishers wanted brought him more conventional literary success than he wanted. Perkoff, a child of the 1930s and 1940s, was the man Lipton yearned to be, an unorthodox gifted poet who lived the Beat life. Perkoff was the Henry David Thoreau of the Venice Beat community, as perhaps Lipton was its Ralph Waldo Emerson.

The book loosely follows Lipton and Perkoff from the 1950s to 1975. They hold center stage, while Venice looms in the background, and national Beat figures such as Jack Kerouac, Alan Ginsberg, and Ken Rexroth briefly filter in and out of the narrative. Lipton and Perkoff are compelling, instructive case studies. Maynard has a fine feel for his subjects and a strong, unpretentious writing style. This might have been merely a literary history of two minor Beat writers, but Maynard makes it a broad cultural study that explores complex connections between Beats and the wider culture.

The personal details of Lipton's and Perkoff's lives are based on their papers and extensive interviews with surviving relatives. Maynard neither debunks nor venerates them. He views them more as representative types than as eccentrics and is a sympathetic, yet critical, observer. He makes the best case for their

importance by noting that, whereas Kerouac wrote about the Beat life of the 1940s, Lipton and Perkoff established the Beat images of the 1950s. It was these later images that often moved and informed 1960s activists.

Maynard is often perceptive about connections between Beat images, ideas, and style and the 1960s counterculture. Unfortunately, this commentary is scattered through the book and does not form a thesis. A concluding chapter summarizing these ideas would have helped. Despite organizational flaws, the book's bold, skillful narrative and willingness to reach far beyond its immediate subjects toward national significance make it both irresistible and important.

JERRY RODNITZKY
*University of Texas,
Arlington*

BARBARA EPSTEIN. *Political Protest and Cultural Revolution: Nonviolent Direct Action in the 1970s and 1980s*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xiii, 327. \$24.95.

This book analyzes and celebrates an important current of contemporary social activism that until now has not received the attention it deserves, the nonviolent direct action movement. Barbara Epstein addresses in particular its antinuclear campaigns in the 1970s and 1980s: the Clamshell Alliance against the Seabrook nuclear plant in New Hampshire, the Abalone Alliance against the Diablo Canyon nuclear plant, and the Livermore Action Group's efforts to shut down the nuclear weapons-producing Lawrence Livermore Laboratory, the latter two in Northern California. She writes as a participant in these movements, perhaps even a convert from more familiar forms of leftist politics. Although she is not totally uncritical, Epstein enthusiastically embraces the political sensibilities and goals of pacifist direct action politics. Two chapters ("Feminist Spirituality and Magical Politics" and "The Religious Community: Mass Politics and Moral Witness") convey both the integrity and courage of those involved in antinuclear direct action politics (and nonintervention policies in Central America) and her own admiration for and feelings of connectedness to them. It is somewhat perplexing to find such marginal movements as paganism, witchcraft, anarchy-feminism, and Christian pacifism treated as though they were central to social change in America. Epstein clearly believes that despite their marginality, their example is important; she thinks that the attempt to develop a politics of cultural revolution is required of those engaged in democratic social change.

The strengths of these movements, according to Epstein, lie in their communitarian and utopian approach to politics, where the goal is not only to change economic and political structures but also to create equal, nonviolent, and ecologically sound social

relationships. The specific objectives of those in the direct action movement are always inseparable from the creation of alternative social relationships, a form of cultural revolution that has as its aim a decentralized society based on participatory democracy at the community level. Hierarchies of power are rejected in favor of anarchist, antiauthoritarian principles in which everyone participates equally in decision making and political activity.

The nonviolent direct action movement is weak on political strategy, and of this Epstein is critical. Her last chapter, "Radical Politics in Late Capitalist Society," is an exceedingly useful evaluation of the significance of new social movement theory and postmodernism for understanding contemporary social movements. She argues that the lack of concern with strategy in analyses of social movements in Western advanced capitalist societies and in social movements themselves, a lack the direct action movement shares, is a major shortcoming for those who want to transform capitalist society. This is the central criticism of utopian politics by realists who espouse organization, from Leninists to mainstream sociologists. Epstein's strategic concerns are puzzling in light of her appreciation of explicitly unstrategic nonviolent direct action politics, since a central component of the latter is precisely a critique of building organization and strategizing in the conviction that they will lead to hierarchy and conflict.

In this otherwise useful book, there is almost no acknowledgement of the New Left and movements of the 1960s, including the nonspiritual women's movement, in the development of some of the central ideas of the nonviolent direct action movement of the 1970s and 1980s. It is as if the building of community, consensus, participation, spirituality, counterinstitutions, and exemplary actions in the pacifist, direct action movements of the 1970s and 1980s had no roots in earlier movements, either in the immediately preceding years of the 1960s or throughout the postwar period and before. Books such as Nigel Young's *An Infantile Disorder? The Crisis and Decline of the New Left* (1977), Greg Calvert and Carol Neiman's, *A Disrupted History: The New Left and the New Capitalism* (1971), and my own work on the early New Left, *Community and Organization in the New Left: The Great Refusal* (1989) sympathetically analyzed utopian, communitarian political experiments in the New Left. Young, a pacifist, examined the New Left's retreat from its initial nonviolent impulses. The central tension I explored was that between "prefigurative politics" (a term Epstein uses throughout her book), the attempt to create new egalitarian and participatory social relationships in the process of achieving practical goals, and strategic politics, a more instrumental effort to alter the institutions of society. This is the same tension she discovers and explores in the nonviolent direct action movement. Yet none of these earlier analyses and histories enter Epstein's discussion. Epstein, then, succeeds in giving recognition

and attention to the nonviolent direct action movement but inexplicably glosses over the significance of the political experiments that preceded it.

WINI BREINES
Northeastern University

NORMAN E. SAUL. *Distant Friends: The United States and Russia, 1763–1867*. Lawrence: University Press of Kansas. 1991. Pp. xvi, 448. \$40.00.

We have an expanding library of monographs, memoirs, journal articles, and documents on the first century of Russian-American relations but, until now, no detailed survey that would digest these disparate works. John Lewis Gaddis pointed the way with his sweeping "interpretation" of the entire course of relations (*Russia, the Soviet Union, and the United States: An Interpretive History* [1978]). As balanced and revealing as that book was, his treatment of the eighteenth and nineteenth centuries touched only the high spots of diplomacy as a preamble to events of the mid-twentieth century. By contrast Norman Saul promises three volumes just to bring the story up to the 1920s. This would seem to be overkill if Saul wrote traditional diplomatic history, but the great strength of this first volume of the projected trilogy lies in its breadth of subject. It encompasses nothing less than "what is known about the Russian-American experience," and it succeeds. The work centers on political relations but devotes substantial attention in every chapter to commercial exchanges, technological connections, and cultural interaction. Indeed, the most original portions of the book are those describing Russian perceptions of American society and American mercantile enterprises in St. Petersburg, not the diplomacy of the Dana Mission or the Alaska Purchase.

To achieve such a comprehensive treatment Saul has gone far beyond the convenient published sources, which are quite extensive themselves, to archival material here and (to a lesser extent) in Russia, and into a mass of manuscript collections. Early chapters rely heavily on documents in *The United States and Russia: The Beginning of Relations, 1765–1815* (1980) and the pioneering work of N. N. Bolkhovitinov. But to capture the quality of personal contact between Russians and Americans in this period, Saul exploits the family correspondence of Boston shippers, business records, travel memoirs, and newspapers. Saul's bibliography is unrivaled, particularly in its citation of private manuscript collections in the United States.

By adopting a definition of relations that includes traditional diplomacy, economic interests, and personal experiences, Saul creates some organizational problems. The course of diplomatic negotiations leading to the formal exchange of representatives, for example, is interrupted while the author tells the story of Russian emigrants to America and describes

the nature and extent of commerce. But what one occasionally loses in continuity one gains in perspective. A deeper pattern of relations emerges, one that derives substantially from national interests expressed by state officials but is also influenced by trade and private citizens. Saul believes that the harmonious relationship that these "distant friends" developed in the nineteenth century depended on feelings of mutual interest among the educated public in both countries as well as the perception of a common enemy (Britain) and the absence of territorial conflict, the key factors Gaddis and others have emphasized.

Nevertheless, Saul does not pursue a dramatic reinterpretation of Russian-American relations. The major diplomatic events, from the exchange of representatives in 1809 to the sale of Alaska, turn on considerations of national political interest. But he supplements standard interpretations by arguing that commerce, technology, and, at times, cultural affinity reinforced such decisions. Saul is only partially successful in explaining just how important these secondary influences were. He seems to regard trade as the key to entente in mid-century, when he calls it "the most important, continuous, harmonious Russian-American activity" (p. 181), but the same paragraph notes that mutual trade actually declined in relation to the growth of the two economies in the period. His description of the role of technology in deepening contacts is more consistent and convincing. Saul's survey reveals a substantial pattern of technical interaction in matters of naval design, railroad construction, and munitions between Russians and Americans. Some of these avenues have been explored by American historians such as Joseph Bradley (*Guns for the Tsar* [1990]) but many have not and should be.

Indeed one of the primary contributions of this thorough and well-written survey is that it contains a mine of sources and ideas for further research. He suggests topics in business history and introduces us to intriguing individuals such as William Ropes and Nancy Prince Gardner.

In short, Saul offers us the most comprehensively written and thoroughly researched study of the first century of Russian-American relations ever presented and a fundamental resource for anyone pursuing research in the field.

RONALD J. JENSEN
George Mason University

WARREN F. KIMBALL. *The Juggler: Franklin Roosevelt as Wartime Statesman*. Princeton: Princeton University Press. 1991. Pp. xii, 304. \$19.95.

Franklin Roosevelt was hard to figure out when he was living, and he is not much easier dead. Roosevelt's policies varied by the month, although whether the varying reflected an absence of a fundamental phi-

losophy of governing or self-conscious tactical zigzags is difficult to determine. Warren F. Kimball thinks it was the latter. Kimball says Roosevelt did know what he was doing, at least when it came to U.S. policy during World War II, even if no one else did. According to Kimball, Roosevelt deliberately deceived those who tried to guess which way he would jump next. "I may be entirely inconsistent," Kimball quotes Roosevelt. "I am perfectly willing to mislead and tell untruths if it will help win the war" (p. 7).

Kimball finds it trickier to specify just what Roosevelt's guiding philosophy was than to state that he had one. Roosevelt is no help; the thirty-second president left almost as few tracks for historians as for contemporaries. Finding little in the way of direct evidence, Kimball falls back on the rule that philosophy is as philosophy does. Kimball infers from Roosevelt's actions three basic "assumptions" regarding America's place in the international order: first, that World War II witnessed a critical shift in the balance of European power, from the Western countries toward the Soviet Union; second, that the liberal tenets of the New Deal were a model for stable and peaceful global relations; and, third, that Wilsonian idealism had failed not because it misjudged human nature but because Wilson applied it inflexibly. Kimball labels this set of assumptions "Americanism"—not an especially helpful tag, since it implies that those many persons (from Herbert Hoover to Ronald Reagan) who did not share Roosevelt's views were un-American.

Kimball concedes that discerning Roosevelt's assumptions in his day-to-day maneuverings is not always easy. Roosevelt never held policy hostage to principle; as Kimball charitably puts it, "Roosevelt saw no sense in letting ideology stand in the way of common sense" (p. 185). Kimball concentrates particularly on the president's wartime policies toward the Soviet Union and Britain. Regarding the Soviets, Roosevelt understood the challenge that Soviet communism posed to his vision of an open, liberal, democratic world; but he also understood that in the short term Nazi Germany posed a greater challenge. As to the British, Roosevelt believed that their imperialism might in its own way threaten the peace that would follow the war as much as Soviet communism would: each tried to close off parts of the planet from the healthy give-and-take of economic and political competition. Yet Roosevelt did not consider the middle of a major war quite the moment to insist on the decolonization of Britain's empire.

Kimball's book consists of essays written for various audiences and occasions over the last twenty years. Kimball obviously has thought long and hard about his subject (he is the editor of three volumes of correspondence between Roosevelt and Churchill, and the author of two additional books on World War II), and it is instructive to see how his thoughts have changed during that period. Sometimes Kimball gives the impression that he has Roosevelt pinned down;

sometimes he seems to despair of ever understanding the "sly squire of Hyde Park" (p. 7). One cannot help thinking that if Kimball keeps after Roosevelt for another twenty years, his mind will continue to change. This would be just as well. Roosevelt was a slippery character. Kimball comes as close to success as anyone is likely to in trying to both catch the character and show the slipperiness.

H. W. BRANDS
Texas A&M University

GREG RUSSELL. *Hans J. Morgenthau and the Ethics of American Statecraft*. (Political Traditions in Foreign Policy Series.) Baton Rouge: Louisiana State University Press. 1990. Pp. 258. \$27.50.

Hans J. Morgenthau was one of the most important professors of politics in the United States in the twentieth century. The study and practice of international politics would have been far different had Morgenthau not written his many books, lectured, testified before Congress and taught scores of graduate students. He helped create the realist school of international relations, and his influence among students and practitioners of diplomacy, diplomatic history, and foreign policy was enormous from World War II until the 1970s. During the angry debates over the wisdom and morality of U.S. involvement in the war in Vietnam, Morgenthau's principled opposition to the course of American foreign policy encouraged other opponents with his insights and his intellectual toughness. He also sent tremors through the White House. Johnson-administration officials worried that his criticism of their Vietnam policy was so profound, detailed, and rigorous, that they mounted Operation Morgenthau to discredit him.

All of this makes Morgenthau the perfect subject for a biography that places his career in intellectual context and demonstrates the interplay between his thoughts and writing and the behavior of the people he influenced. Greg Russell does a good job in explaining the development of Morgenthau's ideas. His discussion of the way in which Morgenthau gradually distinguished his theories of realism from Machiavellianism or *raison d'état* is especially well done. Russell knows the literature on the philosophy of European statecraft well, and his description of the Americanization of Morgenthau's ideas is an excellent contribution to the historiography of the trans-Atlantic intellectual migration.

Russell's book is an intellectual portrait of its subject, not a full-scale biography of Morgenthau. The author concentrates on the development of Morgenthau's ideas, leaving aside details of his life as a teacher. Russell asserts Morgenthau's influence, and most readers of this book will be aware that he joined with Reinhold Niebuhr and George Kennan to make political realism the dominant mode of thinking about international relations for twenty years after

1945. Russell has two good chapters on contemporary foreign policy developments and the importance of the American realist tradition in foreign affairs. In them he outlines what Morgenthau said and wrote about current events. What is missing from this account is the richness of detail of Morgenthau's relations with other realists and, perhaps even more importantly, his students.

Readers are left wanting more details than Russell provides. The discussion of the development of Morgenthau's position on U.S. policy during the Vietnam War especially could profit from greater detail and explanation of the extent and limits of Morgenthau's influence. Russell twice mentions that the Johnson administration attempted to discredit Morgenthau, but he does not provide the details. Who was involved? Whom did they contact to discredit Morgenthau? What effect did their criticism have? Similar questions arise regarding Morgenthau's importance as a teacher. Who were his students and how did they develop his realism?

These questions aside, Russell has provided a useful introduction and explication of the thought of a professor who helped define the contours of the relationship between the academy and the state in the contemporary United States.

ROBERT D. SCHULZINGER
University of Colorado,
Boulder

CHESTER J. PACH, JR. *Arming the Free World: The Origins of the United States Military Assistance Program, 1945-1950*. Chapel Hill: University of North Carolina Press. 1991. Pp. x, 322. \$34.95.

"Can we ever stop once we start?" So asked John O. Bell as he prepared in 1949 to help direct the Mutual Defense Assistance Program, the nation's first coordinated arms aid effort in peacetime (p. 227). Its precedent was pathbreaking. By the end of the 1980s, the United States had armed nearly the entire free world—about 120 countries—by grants in excess of \$90 billion in military matériel and training. The program's goals have included curbing aggression, containing communism, winning foreign cooperation, and protecting foreign interests short of sending troops. The genesis and implementation of this program comprise the subject of this finely crafted study by Chester J. Pach, Jr.

The main objective of military assistance, Pach argues, was symbolic: to reassure America's friends of its resolve and thus raise morale in their continuing struggles against communism. The White House therefore integrated military aid into foreign policy as a vital instrument of containment. Such an assistance bill did not come easily. Bureaucratic infighting took place between the State Department and the Pentagon, both of whom sought to safeguard the nation while worrying about an invasion of its do-

main. Furthermore, the State Department lengthened the list of potential aid recipients by emphasizing the political and psychological impact of military assistance, whereas the armed services kept the list short by stressing strategic considerations and warning against overextending foreign commitments or depleting resources for domestic rearmament efforts. Although the final program combined these political and strategic objectives, the administration of Harry Truman never focused on how to relate both the amount and duration of assistance to raising foreign morale and establishing American credibility. This poorly conceived strategy bequeathed a legacy of arms assistance that took on a life of its own, leaving unresolved the problems of determining how much aid was enough, how long the assistance should last, and how to reduce or bring it to an end without suggesting weakness or lack of resolve.

It is refreshing to read a book on the Cold War whose preoccupation is to promote understanding rather than some school of thought. Pach's subject would be especially vulnerable as an ideological football, particularly in light of America's numerous military interventions in succeeding years. Yet he has not made a polemical statement or tried to advance some hidden agenda. Instead, Pach has undertaken solid research, constructed an intricate narrative, and offered a calm analysis of a subject with great emotional potential. Indeed, his work supports the revisionists who have accused the Truman administration of establishing precedents for military intervention through the Truman Doctrine, NATO, and Military Assistance Program; but his analysis departs from their timeworn argument that the White House became the cynical progenitor of a global program of reckless military involvements that led to Vietnam. Although admitting that the administration exaggerated the Soviet threat, Pach notes that its perceptions resulted from miscalculations and a failure to place restraints on a program whose primary objectives were inherently vague, open-ended, and probably not attainable.

Pach has masterfully pieced together an interesting story that does not cast judgments about motive and is dispassionate and fair. By moving beyond the simplistic attribution of blame, he has provided an understanding of how and why America's military assistance program of the late 1940s pointed the way to even more dangerous involvement in the years ahead.

HOWARD JONES
University of Alabama,
Tuscaloosa

GEIR LUNDESTAD. *The American "Empire": And Other Studies of U.S. Foreign Policy in a Comparative Perspective*. New York: Oxford University Press or Norwegian University Press, Oslo. 1990. Pp. 214. \$29.95.

This book by Geir Lundestad is a collection of four essays on U.S. foreign policy since World War II. Three of the essays were previously published. The fourth and longest essay shares the book's title and was specifically prepared for this volume.

The first essay, originally published in 1989, is concerned with the way traditional American views of moralism, presentism, and exceptionalism influenced scholarship on the Cold War. Lundestad calls for an end to the resulting provincialism, which he defines as a "belief in the capacity of the United States to determine events in different parts of the world" (p. 21). He calls on historians to concentrate instead on "more comparison and attention to the interplay between international and local factors" (p. 28). Thus, he joins those historians who have recently called for international histories of the Cold War era.

The other two previously published essays address the pendulum swings in U.S. foreign policy and the origins of the Cold War in Eastern Europe. The latter essay offers little new information or interpretation. The former, however, attempts an analysis of U.S. domestic policy to explain the pendulum swings in foreign policy that have always seemed greater than those in Europe. In this essay, published in 1986, Lundestad offers the interesting perspective of a European observer.

The most important and original essay in the book is the second one, "'The American Empire' 1945-1990." Here the author makes two contributions to the continuing analysis of the last forty-five years. First, he seeks to link the "economic dimension" (p. 94) with the political and national security aspects of U.S. policy. Second, after briefly examining other empires, he offers the view that, unlike its predecessors, the American "empire" after World War II was an "empire" by invitation. Lundestad, of course, is working within a very broad definition "where empire simply means a hierarchical system of political relationships with one power clearly being much stronger than any other" (p. 37). He concedes that not all interventions were by invitation, but since his emphasis is on the relationship between the United States and Western Europe, he does not dwell on these uninvited interventions that were primarily in the Third World.

Lundestad argues that through its emphasis on capital investment in European countries, its encouragement of European unity, and its emphasis on free trade, the United States created the circumstances that would destroy its hegemony and lead to its decline. But he emphasizes that this American decline is relative rather than absolute. The United States, he notes, is still likely to be "Number One" for the foreseeable future.

In recent years the international aspects of U.S. foreign policy during the Cold War have become an increasingly fertile field for analysis. Lundestad seems to associate himself with this movement, but, in fact, the essays in this book are as ethnocentric as any

written by American historians. With some notable exceptions his sources are American, and he is responding to issues and ideas offered by American historians and analysts. These essays, however, contribute to the literature on the Cold War by providing a three-dimensional view that includes the often neglected economic dimension. In addition, Lundestad's elaboration of the "empire by invitation," a concept he first explored in 1986, will continue to be a provocative approach to the ongoing discussions of the Cold War and its demise.

ANNA KASTEN NELSON
American University

PETER L. HAHN. *The United States, Great Britain, and Egypt, 1945–1956: Strategy and Diplomacy in the Early Cold War*. Chapel Hill: University of North Carolina Press. 1991. Pp. xii, 359. \$37.50.

Peter L. Hahn's impressive study richly details events of the crucial decade in the formation of U.S. Middle East policy. He has crafted the best study we are likely to have for some time on the complex diplomatic maneuvers centering on Egypt, the key to the Arab Middle East. His account deftly analyzes the triangular relationship that repeatedly distracted Great Britain's prime ministers, eventually driving the unfortunate Anthony Eden from office in January 1957.

According to Hahn, as the Cold War developed, U.S. officials confronted a continuing dilemma in the Middle East. They said they favored self-determination for Egypt, and yet their concern for regional security and stability led them also to support British control of the Suez Canal, which angered Egyptians. Sometimes the United States pursued irreconcilable objectives, antagonizing both London and Cairo. But at critical moments throughout the decade, policy makers opted for security (and Britain).

If I read him correctly, Hahn believes that Washington, albeit regrettably, had no option but to do what it did, in the interests of "alliance diplomacy and Cold War security" (p. 130). I am not so sure. Did U.S. policy make the Middle East more secure? More stable? What if, in Egypt and elsewhere, American policy makers had been able to look on nationalism not as a disruptive element, opening the way for Soviet penetration, but rather as a force for stability? What might have happened in the Third World—in Iran, in Vietnam, in Guatemala—had they shown less skepticism? To choose this alternative, of course, officials would have had to inform themselves better about the complexities of local politics. This was their weakness. In Egypt, for example, they focused on the Wafd Party as if it alone could deliver an Anglo-Egyptian agreement on Suez when in fact it had lost its credibility by 1945. And to hypothesize that Egypt might have joined a Western-dominated Middle East command if only Britain and the United States had slightly altered their positions seems unrealistic, given

Egyptian Anglophobia. As Hahn occasionally points out, American officials could be intensely naive where Egypt was concerned.

Hahn cites a lesser factor driving U.S. policy: American domestic politics. The pro-Israel lobby used its considerable influence to block arms and economic assistance to Cairo. President Harry S. Truman's concern for Israel, Hahn reminds us, did not cease with independence in 1948. He kept a watchful eye on developments and intervened repeatedly to ensure that the Arabs did not gain any advantage over Israel as a result of American assistance. The administration of Dwight D. Eisenhower tried to be more balanced, but Israeli sympathizers continued to be effective, especially regarding the decision to withdraw the Aswan Dam loan.

In the continuing debate over who made foreign policy in the Eisenhower administration, Hahn presents convincing evidence that, at least until 1956, Secretary of State John Foster Dulles held center stage in the formation of U.S.–Middle East policy. Hahn is perhaps at his best in the penultimate chapter, on events leading up to Suez in 1956, where he succinctly and lucidly dispels several stubborn myths regarding what was arguably the most severe crisis of Eisenhower's presidency.

Overall, his judicious and carefully reasoned study leads us to hope that other historians of U.S. foreign relations will turn their efforts toward the Middle East, where so many issues remain unexplored.

JAMES F. GOODE
Grand Valley State University

MICHAEL J. COHEN. *Truman and Israel*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 342. \$24.95.

Michael J. Cohen starts and ends with the assumption that Harry S. Truman was a racist and an anti-Semite, but he does not substantiate his claim. What is the basis for his claim that Truman was anti-Semitic and racist? In a letter to his wife Bess, he supposedly wrote, "I have a Jew in charge of the canteen by the name of Jacobson and he is a crackerjack. Also the barbershop is run by a Jew, Morris Stearns by name." This makes him an anti-Semite, a racist? Such distortions make me wonder what motivated the author to write a book on Truman. Cohen conveniently chooses to ignore Truman's stand against the KKK when he served as judge in Jackson County, Missouri. Truman hired those who were qualified, willing, and able to work, regardless of race, religion, or nationality. The author disregards the fact that the Klan crusaded against Truman and helped defeat him in one of his attempts at reelection for judge. He ignores the fact that Truman fought against discrimination, desegregated the armed forces, and attempted to ensure against discrimination in any federally sponsored project. Truman insisted on a civil rights plank in the

Democratic Party platform of 1948 and introduced the first civil rights legislation since Abraham Lincoln and Andrew Johnson, which Congress rejected.

Cohen fails to understand the background, personality, and motivation that made Truman one of America's greatest presidents. He recounts the story of how individuals in the Truman administration opposed the creation of the state of Israel but does not explain what motivated them. Was it old-fashioned Jew hatred? Was it vested interests in the military-industrial complex entangled with Arabian oil? Was it their belief that the British empire should flourish in the Middle East?

Except for Cohen's reference to Truman's respect for Secretary of State George Marshall, he presents little understanding of Truman's view of the presidency, the Constitution, and the institutions of the American democratic republic. Nor does he have a good grasp of Truman's view and knowledge of history.

Although Cohen reviews State Department tactics against the Jews and the Jewish state, which he found in secondary works and in primary sources from the National Archives, his work is basically a rehash of what has already been published in a variety of articles and books.

Cohen thanks the Israel State Archives for the use of its materials, but there is little reflection of his having incorporated the contents of this archive in his work. Nor is there evidence that he used their publications (*Documents on the Foreign Policy of Israel*), although he cites one of them in his bibliography. He neglects a wealth of information that illuminates Truman's decision to recognize Israel, the U.S. arms embargo against Israel, Truman's concern over the Soviet Union's expanding influence, and U.S.-Israel relations in general. Had he made use of published and unpublished Israeli documents, he would have learned about such intervening Jewish America Firsters as Henry Morgenthau, Jr., Senator Herbert Lehman, and American Jewish Committeeman Jacob Blaustein, who insisted that Israel abandon its neutrality and side with the United States in its Cold War struggles. These documents help the historian understand the complexity of the U.S.-Israel relationship.

Equally appalling is Cohen's remark that Truman "had no apparent appreciation of the historical significance of the Holocaust" (p. 276). Truman insisted that Nazis be put on trial so that history would record their crimes and so that Hitler would not be considered just another Napoleon. At the Harry S. Truman Presidential Library there are materials revealing Truman's appreciation of the Holocaust. In a series of television programs ex-President Truman devoted one entire show to the Holocaust and Israel. He recalled having seen what the Nazis had done to the Jews and said that he often relived those scenes in nightmares.

The essence of President Truman, the person, does not appear in this book. His feelings for humanity,

feelings for the Jewish people, concern for the spread of Soviet totalitarianism, and struggle to preserve the presidency from State Department officials who sought to undermine it has been seriously overlooked by this account.

H. DRUKS

Brooklyn College

LAWRENCE S. KAPLAN. *NATO and the United States: The Enduring Alliance*. (Twayne's International History Series, number 1.) Boston: Twayne. 1988. Pp. xvi, 237. \$24.95.

Lawrence S. Kaplan's study should become the standard work on NATO at its zenith. The volume is brief, readable, and provides a good balance of narrative and interpretation covering the alliance from its origins up to roughly 1986. As the leading academic specialist on the alliance, Kaplan has directed the Lyman L. Lemnitzer Center for NATO Studies at Kent State University since 1979 and is the ideal person to write such a general history.

In his opening chapters Kaplan reminds us how rapidly the alliance evolved as the Cold War intensified. Initially, the administration of Harry Truman wanted to offer only limited assistance and political assurances of support to encourage the West European states to establish a new institution for defense cooperation. But the events of the winter and spring of 1948—the Czech coup, Communist threats to Italy and Norway, and the Berlin blockade—broke down fear of cooperation on both sides of the Atlantic. The signing of the North Atlantic Treaty in April 1949 was a revolution in U.S. foreign policy, because of the government commitment to aid our European allies if they were attacked. In its original form, NATO was a political alliance with coordinated defense planning for Europe and a military staff, but it was not a functioning military alliance. The events that flowed from the Soviet detonation of an atomic device in August 1949 and the outbreak of the Korean War ten months later transformed NATO into a full-fledged military alliance with an extensive deployment of U.S. combat forces in Western Europe.

The alliance continued to develop in response to a series of dramatic events. Sputnik, the Berlin crises, French withdrawal from the alliance's integrated military command, and the Euromissile crisis beginning in 1978 each left its mark on alliance political commitments and force structure. In addition to charting clearly the development of the alliance as a political and military institution, Kaplan usefully shows how various steps of alliance evolution affected the movement for European economic and political integration. He shows how NATO provided the essential umbrella beneath which France, Germany, and the United Kingdom could develop unprecedented levels of supranational cooperation. But, at the same time, the central U.S. role in the alliance functioned to

retard the pace of European integration by bringing states such as Portugal, Norway, and Italy into the alliance, by insisting that military assistance be bilateral in nature, by providing the preponderance of nuclear forces that represented the highest level of military power of the alliance, and by having Americans serve continuously as supreme commanders of the military organization.

Since this book was written, the Cold War has ended and the Soviet Union has disintegrated. It is very likely that NATO will survive these changes, but it will again become essentially a political alliance. The great value of Kaplan's book is that it captures the dynamics and successes of NATO during the height of the Cold War.

SAMUEL F. WELLS, JR.

*Woodrow Wilson International Center for Scholars
Washington, D.C.*

MARTIN STANILAND. *American Intellectuals and African Nationalists, 1955–1970*. New Haven: Yale University Press. 1991. Pp. 310. \$30.00.

Martin Staniland's intriguing narrative blends American intellectual and diplomatic history. Moving beyond the purely domestic focus of most intellectual history, he unravels superbly the polemics of American intellectuals in their writings on Africa as the Cold War peaked.

Despite its thematic content, the book, Staniland explains, is less about international relations and more about "the images of foreign societies that shape our attitudes and policies toward them" (p. 1). He charts those images from the nineteenth century when African Americans Martin Delany and Henry McNeal Turner devised African repatriation plans. Yet African-American diplomat John Smyth lamented the generally disparaging images of Africa as an "ancient and mysterious land" held by Americans both white and black in the 1890s (p. 11).

In the twentieth century, African-American intellectuals Carter Woodson, W. E. B. Du Bois, and Paul Robeson revered their African heritage while Marcus Garvey resurrected dreams of African repatriation. But by the 1950s Africans were still being described as "Kru apes," a reference to Kru stevedores from Liberia, and a *Time* magazine article on the Belgian Congo (Zaire) declared that "sons of cannibals now mine the raw materials [uranium] of the Atomic age" (pp. 65, 218).

Staniland argues that intellectual and diplomatic historians have investigated such imagery, but their work has had seemingly little impact on the political scientists who write about foreign policy (p. 1). He explores instead the landscape of American political culture as he probes the images liberals, conservatives, and radicals constructed of Africa and their impact on policy makers (p. 4).

Staniland examines the images of Africans pre-

sented by international scholars, beginning his study by describing the liberals. "Institutional nationalists" such as Melville Herskovits, Gwendolen Carter and African-American Martin Kilson, a "democratic evolutionist," argued that Africans had to evolve their own democratic institutions (pp. 62, 83–86). "Nation builders" like Immanuel Wallerstein advocated building strong and even autocratic African states (pp. 79, 88–89). "Modernizationists" Chester Bowles, Adlai Stevenson, G. Mennen Williams (all associates of President John Kennedy), and Senator Hubert Humphrey saw African nationalists as modern heirs of the Enlightenment and to America's own anticolonial revolution. The more traditionalist liberal Grant McConnell countered that they misconceived the 1776 analogy to African nationalists and that American colonists were more analogous to European settlers in Kenya and Rhodesia (pp. 76–77, 80).

Vivian Drake, like most conservatives, rejected the use of the 1776 revolutionary metaphor. She reasoned that the Americans were enlightened men while African nationalists were barely civilized (pp. 231, 229). But Staniland sees contrasts too among conservatives. Traditionalist James Burham thought African independence had come too fast and that decolonization was the West's display of "moral funk and mental stupor" (p. 225). Free enterprise, market-oriented conservative Elliot Berg worried about threats to individual enterprise through nationalization of economic resources. If liberals universalized their political vision, then many conservatives accepted Reinhold Niebuhr's belief in a hierarchical view of societies, reasoning that pluralism, parliamentary democracy, and competitive politics were exceptionally suited to the United States and not relevant to Africa (pp. 87, 216).

Radicals also rejected application of the 1776 metaphor, arguing that it was a bourgeois revolution with few lessons for Africa. They condemned colonialism but attacked the term when African-American nationalists John Henrik Clarke, Harold Cruse, or Malcolm X suggested that black Americans were victims of internal colonialism (pp. 147, 192). Most radicals repudiated capitalism, preferring collective control of the major means of production and distribution of society (p. 9).

American intellectual discourse on Africa moved to a more substantive level, however, after the 1955 Bandung, Indonesia meeting. In a unique analysis, Stevenson claimed that the Afro-Asian Conference revealed that "no event in human history [had] said more to the peoples of Asia or Africa, or provided more nourishment for the moral imagination, than what happened in America from 1776 to 1789" (p. 76). But the presence of Soviet delegates at Bandung led to Secretary of State John Foster Dulles's remark that colonial territories had been targeted by international communism and that they would be absorbed into the communist bloc once nationalists got rid of colonialism (p. 245). Although State Department lib-

eral Anthony Harrigan questioned the strategy, most liberals approved a U.S. policy to protect moderate anticommunist African nationalists to win friends in the Cold War (p. 226).

Staniland states that Ghana's nominal independence in 1957 led by Prime Minister Kwame Nkrumah attracted the attention of such intellectuals as black scholars Shirley Graham DuBois or historian Chancellor Williams, who reminisced about the grandeur of ancient Ghana although it was some distance away from the new state. As Nkrumah expressed a more Pan-Africanist view, detained opponents, and talked with the Soviets, such intellectuals as Joseph Nye remarked that many liberals had been too gullible and "accepted nationalist slogans at face value" (p. 101). Defenders of Ghana's government pointed out, however, that Africa in the 1960s had similarities to the United States in 1790s with its Alien and Sedition laws, oppression of native tribes, official corruption, and enslavement of Africans (p. 94).

Staniland dispels negative criticism that intellectual perspectives are ethnocentric. He defines ethnocentrism nonpejoratively, insisting that all individuals have "a set of mental templates" enabling them to construct meaning from their complex societies (pp. 2-3). Staniland documents his text carefully and relies especially on Clifford Geertz's work on ideology and cultural systems as well as Michael Hunt's writings on ideology and foreign policy.

He is overly sharp, however, when he writes: "Having surveyed some of the damage that Africa did to alien ideologies, we should ask in conclusion what we can learn from this case about the role of ideologies in the making of foreign policy and in international relations more broadly" (p. 275). Staniland is probably right, however, when he notes that to go beyond intellectual history requires a sociology or ethnography of international relations.

KATHERINE HARRIS
Indiana University of Pennsylvania

J. VALERIE FIFER. *United States Perceptions of Latin America, 1850-1930: A "New West" South of Capricorn?* New York: Manchester University Press, distributed by St. Martin's. 1991. Pp. vii, 203. \$49.95.

J. Valerie Fifer has produced a useful, if not completely satisfying, study dealing with the optimistic first impressions and subsequent disappointments expressed by U.S. policy makers, businessmen, and travelers concerning the economic development of Latin America, particularly Argentina and Chile, from the mid-nineteenth century to the 1930s. Early optimism was fueled by the North Americans' perceptions of the physical similarities between the southern cone nations and their own nation's West. In their minds, the pampas of Argentina, the imposing Andes, and the mineral-rich coastal areas of Chile were roughly equivalent to the more familiar broad

midwestern plains, Rocky Mountains, and California coastline. All that was needed, in their opinion, was for those nations to follow the prosperous example of the United States: construct a vibrant railway system, opening up the interior and linking the coasts; encourage immigration; and devise a land program to pull those immigrants into the interior to develop it and provide even more markets.

The results in Latin America, however, were disappointing. Uninformed and/or uninterested government officials and conservative British railway companies combined to stifle the development of truly national (and transnational) rail systems. The governments of Argentina and Chile never put together a systematic program for attracting and keeping immigrants. And, despite sporadic efforts, no effective system for land grants was ever established. By the 1920s, U.S. officials and entrepreneurs more often compared the region to the Old South than the New West.

Fifer has unearthed some interesting information. Her account of the official U.S. surveys of Latin America done in the 1850s does much to dispel the notion that U.S. interests in that region were confined to Mexico, the Caribbean, and Central America. A long second chapter provides some keen insights into the battle for railroad concessions waged between U.S. and British interests during the mid-nineteenth century. Throughout the book, Fifer does a good job of evoking mental pictures of nineteenth-century life in those nations.

Despite all of this, however, the thematic foundation of the work is shaky. Fifer's thesis that by the 1920s the early optimism concerning the perceived similarities between the United States and Latin America was dashed on the hard rocks of the "reality and the geographical extent of the Latin American culture region" (p. 183) is suspect on several levels. The "U.S. perceptions" are narrow—primarily a handful of entrepreneurs, railroadmen, and consuls. How much impact did these men have on policy makers in Washington? How widespread were such perceptions beyond this small circle of men? Fifer's implication that actions such as the construction of national railroad systems would have cemented positive U.S. views of Latin America ignores the deeper ideological constructs of policy makers in Washington. Recent works by Michael Hunt and John J. Johnson outline their grave concerns about the racial, political, economic, and social make-up of the peoples to the south; concerns that would probably not be erased with the construction of a transcontinental railroad. Finally, Fifer is clearly wrong in suggesting that by the 1920s the vision of Latin America as a New West had been discarded. The speeches and letters of U.S. officials, as well as the business journals of that era, were still full of such references. Of course, they made it clear that this New West would serve much the same function as the old: as an outlet for industrial overproduction and as a rich field for natural

resource exploitation. And perhaps here we can suggest an alternative reason for the disillusionment of which Fifer writes: it was not because the Latin nations were not developing; but rather because they were not developing in the manner best suited to U.S. needs.

Despite these shortcomings, Fifer is to be commended for her evaluation of the sometimes shadowy world of "perceptions" and their role in U.S.–Latin American relations. As is often the case, however, the perceptions about Latin America held by U.S. officials, tourists, and businessmen often end up telling us more about how they viewed themselves than foreign others.

MICHAEL L. KRENN
University of Miami
Miami, Florida

WILLIAM F. SATER. *Chile and the United States: Empires in Conflict*. (The United States and the Americas.) Athens: University of Georgia Press. 1990. Pp. xi, 249. Cloth \$30.00, paper \$15.00.

POUL JENSEN. *The Garotte: The United States and Chile, 1970–1973*. In two volumes. Aarhus, Denmark: Aarhus University Press. 1988. Pp. 304; 305–606.

United States actions in Chile have been controversial for a century, and never more so than during the socialist government of Salvador Allende Gossens (1970–73). U.S. complicity in sabotaging that government and supporting its ouster by Army General Augusto Pinochet Ugarte has generated a voluminous literature. William F. Sater places that episode within the context of the entire history of U.S.–Chilean relations, while Poul Jensen locates that intervention within the full panoply of connections of U.S. government agencies with Chile in the 1960s and 1970s.

Sater gives us breadth, Jensen depth. Both scholars are careful to admit where existing evidence, especially on recent years, is too spotty to draw definitive conclusions. Although cautious and competent, neither author presents significant new research or startling new interpretations. Both agree with the standard view that the U.S. role in President Allende's downfall was less than that claimed by President Richard Nixon's critics and more than that admitted by his defenders. Like most scholars, both of these writers conclude that the United States contribution to undermining Chilean democracy was pernicious but not decisive. Although offering little new, Sater provides a useful general survey, while Jensen furnishes more details—and more analysis of details—than in any other study.

Sater argues that the United States and Chile have been in conflict repeatedly because of their similarities. In his view, both evolved as stable capitalist republics with imperial ambitions over their neigh-

bors. In both countries, those ambitions were fueled by attitudes of racial and cultural superiority.

In the nineteenth century, Chile as well as the United States prevailed over their weaker, less orderly Spanish American neighbors. In those years, Chile and the United States mainly tangled over geopolitical issues, such as Chile's War of the Pacific (1879–84) against Peru and Bolivia. Any hope of competing with the United States evaporated after the humiliating Baltimore incident of 1891, when Chile was forced to apologize for a barroom brawl involving U.S. sailors. After that encounter with gunboat diplomacy, Chilean regional influence faded as the United States and other more powerful nations such as Argentina made gains. Chile was too small in size and had too few resources to remain a contender in South America. No longer a geopolitical challenger even on the west coast of the continent, Chile became increasingly resentful of U.S. hegemony, which was consolidated in the 1920s.

Sater's treatment is strongest on the nineteenth century, but he also displays a sure grasp of developments in the twentieth century. From 1930 on, Chile confronted the United States mainly on the issue of economic nationalism. Although Sater is correct that many Chileans came to blame the United States for their problems of underdevelopment, he overstates the pervasiveness of that point of view, for Chileans also attributed backwardness to many of their own institutions and actions. Their anger over international economic exploitation culminated in the hostility between the Allende and Nixon governments, embroiled in the Cold War. Although highly critical of Allende, Sater adopts a balanced view of U.S. relations with that socialist experiment and the subsequent right-wing dictatorship of Pinochet (1973–90). Sater's only serious mistakes are, forgivably, his predictions about the current government of Patricio Aylwin (1990–94), which he foresees clashing rather than cooperating with the United States.

Sater's coverage of friction between Chile and the United States from the opening years of the nineteenth century to the closing years of the twentieth century is synthetic and solid. Relying mainly on secondary works, it is well argued and well written. In the process of scanning Chile's international affairs, the author also delivers a readable sketch of domestic developments from independence to the present. He has produced the best single survey of U.S.–Chilean relations from the 1810s to the 1980s.

Jensen's study will only appeal to an extremely specialized audience. He drowns the reader in details. Wordy and repetitive, the book lacks a clear conceptual framework. Unfortunately, the cumbersome translation from Danish into English compounds the difficulty of wading through the material.

Jensen is particularly determined to establish that the reports of the investigative committee chaired by Senator Frank Church on U.S. intervention in Chile have not been assessed with sufficient meticulousness.

Jensen's study relies heavily on those congressional hearings, which he reconstructs with excruciating exactitude. He also places under his microscope subsequent accounts by scholars, journalists, and participants, especially Henry Kissinger. Strangely, Jensen rejects conducting any groundbreaking research—especially interviews—of his own, claiming that everything obtainable is already in the published record. The result is a dense, lengthy book supplying a piece-by-piece biopsy of each shred of public information.

After dissecting every iota of existing evidence, Jensen contextualizes his story with a description of general U.S.–Latin American relations in the 1960s and 1970s. He provides another backdrop with an elaborate discussion of the functioning of the complex foreign policy machinery of the U.S. government. The result is certainly the most thorough treatment of this topic in any language. Curiously, this painstaking effort adds little to the general debate. Even after all his detective work, Jensen is reluctant to posit any bold conclusions. He prefers to let his readers reach their own judgments from the “facts.” He states: “what is the historian's limitation in this affair is at the same time the reader's opportunity to indulge his or her imagination” (p. 495).

Like Sater and others, Jensen observes that Nixon and Kissinger tried to destabilize the Allende government from the moment of its election in 1970, principally through economic strangulation. He also notes the CIA's support for the opposition to Allende's administration. And he concludes that it is impossible to establish a direct link between those subversive efforts and the final military takeover. He further argues that U.S. malevolence toward Chile was typical of its animosity toward socialism and nationalism in the Third World, stretching from Nicaragua to Libya. None of these arguments differs from the conventional wisdom. Until fresh documentation is discovered or a deviant interpretation is concocted, the case seems to be closed on U.S. relations with Allende's Chile.

PAUL W. DRAKE
University of California,
San Diego

CANADA

MICHAEL GAUVREAU. *The Evangelical Century: College and Creed in English Canada from the Great Revival to the Great Depression*. (McGill-Queen's Studies in the History of Religion, number 5.) Buffalo: McGill-Queen's University Press. 1991. Pp. x, 398. \$39.95.

The remarkable secularization of Canadian society in this century has encouraged many intellectual historians to reexamine the complex process that dislodged Protestantism from its dominant nineteenth-century position. Recent studies have located the roots of this development in the period between 1860

and 1890 and argued that during these decades, in response to Darwinism and the higher criticism, well-intentioned professors and preachers confidently, but with inadequate intellectual resources, entered into a defense of the faith. By the end of the century theology had been removed from its central place in the college curriculum, leaving the field to the social sciences and a vague theological liberalism, interlaced with philosophical idealism.

This interpretation has not, however, gone unchallenged. Michael Gauvreau's book, the fifth in the series “McGill-Queen's Studies in the History of Religion,” reflects both the current interest in religion and the debate engendered by the secularization thesis. Adopting a highly critical revisionist position, Gauvreau carefully examines the writings of Methodist and Presbyterian clergyman-professors in Canadian colleges and seminaries from 1820 to 1930 and draws attention to the continuity and vitality of an evangelical creed that balanced the claims of faith and reason. Rooted in Scripture and in a transatlantic evangelical restatement of the Scottish Enlightenment, this creed remained open to questions of human nature and society and acted as a bridge between college and pulpit for over a century. Its ultimate demise in the 1920s was caused not by the impact of evolutionary science and the higher criticism but by the challenge that World War I, the social sciences, historical relativism, neo-Kantian idealism, and an increasingly consumer-oriented society posed to its claims to moral and religious certainty.

In his sustained and richly documented analysis of the contours of nineteenth-century evangelical thought within its transatlantic context, Gauvreau makes a contribution that extends beyond the Canadian scene. What distinguished the younger English-Canadian intellectual community from its American counterpart, he argues, is that, while both looked to Baconian or inductive science as a means to inculcate sound attitudes to knowledge and to encourage wide participation in science, Canadian clergymen-professors were influenced by the convergence of revivalism with the early nineteenth-century awareness of the historical. Rather than looking to natural theology to perceive God's design and activity in the world, they turned to the living word of the Bible and the past record of human societies.

This appropriation of the historical led to an enduring synthesis of religion, education, and culture that allowed Canadian evangelicals confidently to encounter evolutionary science, the higher criticism, and the demand for social reform brought about by industrialization and immigration at the turn of the century. As a result, they did not experience the controversies and the split between “modernists” and “conservatives” that troubled the American scene in the final decades of the century and marked the beginning of the fundamentalist movement. This radically different consequence of the Baconian legacy in Canada, Gauvreau suggests, may lead to a

revision of the paradigm presented by George Marsden, America's leading historian of the origins of fundamentalism, by "giving less emphasis to Baconianism, and far more to the persistence of Calvinism and of traditions of Paleyite natural theology and philosophical theology in the older society" (p. 289).

Gauvreau's book, although sometimes disconcertingly opaque, warrants a careful reading and offers important insights to the continuing reassessment of the influence of evangelicalism not only in Canada but also in the United States.

MARGUERITE VAN DIE
Queen's University
Kingston, Ontario

ARTHUR J. RAY. *The Canadian Fur Trade in the Industrial Age*. Buffalo: University of Toronto Press. 1990. Pp. xviii, 283. Cloth \$40.00, paper \$18.95.

Arthur J. Ray is one of the leading modern students of the Canadian fur trade. In this ambitious and pioneering book, he turns his attention to the little-known period from 1870 to 1945, drawing on a mass of relatively recently opened records in the Hudson's Bay Company (HBC) archives. The book deals principally with the vast central subarctic (the Hudson Bay watershed and the Mackenzie River). There is substantial discussion of native culture and economies, public policy, and ecological issues such as "overhunting," but the primary subject is the HBC itself.

The book includes numerous well-chosen illustrations (unfortunately not explicitly referenced in the text), over fifty maps and figures, and over twenty tables. The quantitative data are not always handled with sophistication, however. The reader can spot errors in captioning (figures 16, 17, and 51), recognize and sometimes even recalculate unweighted or otherwise incorrect averages (for example, in tables 6, 13, and 16), use evidence in the book to revise the order of species in tables that rank them by number of pelts rather than by total value (tables 4 and 9, for example, are misleading unless this is done), and discount data presented at inappropriate levels of accuracy (as in Ray's statement that a native family's outfit by the early 1950s cost "approximately \$1,055.03 per year" [p. 201]). More problematic, because they are not discussed, are large inconsistencies between table 9 and figure 29 (knowing the base years for the prices in each might help) and between tables 13 and 17 and figure 39 (post-by-post data on "cash trade" in the tables do not square with totals given in the figure for the same districts). Extreme caution is needed in interpreting data and arguments on profits and returns to the HBC; as Ray recognizes (p. 74, but perhaps not later), terms such as "investment," "capital," and "gross profit margin" reflect HBC usage, not the usual meanings of such terms.

The extensive use of twentieth-century price data would benefit from explicit discussion of how the series incorporate the enormous variations in prices for a single species that table 7 reveals (presumably the variance was one of quality).

Ray's main theme is competition. Arguing on the basis of market share, he contends that the HBC "steadily lost ground" (p. 29). Yet, although data are not given directly, actual HBC fur and retail volumes were evidently much higher in the 1920s and 1930s than before 1905, and, at the end, the HBC was still there, having absorbed or defeated many rivals. The extensive discussion of cash buying leaves the reader surprised by the low proportion of furs that the HBC actually acquired for cash. In such matters, and despite its extensive discussion of the overall structure of the trade, the book sometimes appears too closely immersed in internal HBC data and too inclined to prefer anecdotes to the more nuanced evidence of its own statistics.

The excellent conclusion, "The Decline of the Old Order," seems to suggest that Ray was premature in finishing his book on the fur trade to 1870 (*Indians in the Fur Trade* [1974]) with a chapter entitled "End of a Way of Life." Explicitly linking the two arguments might have resolved the apparent inconsistency. On this as on other issues, it is to be hoped that this book spurs debates as fruitful as those to which Ray's earlier work was so central.

DOUGLAS MCCALLA
Trent University

JOHN ENGLISH. *Shadow of Heaven: The Life of Lester Pearson*. Volume 1, 1897–1948. Toronto: Lester and Orphen Dennys. 1989. Pp. xi, 414. \$28.95.

Lester Pearson served from 1963 to 1968 as Canada's fourteenth prime minister. He was also Canada's foremost diplomat and the luster that will always attach to his name derives less from his prime ministerial career than from his accomplishments in Canada's fledgling diplomatic corps and, from 1948 to 1957, as secretary of state for External Affairs. The period covered by John English's fine biography deals with the formative years of Pearson's life and his emergence as the country's preeminent diplomat.

In setting out to reconstruct these years, English faced some formidable challenges. Pearson was a reticent man who revealed little of himself even in private correspondence, and a good deal of what was to be said had already been covered in his own memoirs. There was also the problem of placing Pearson's career in the context of the unfolding history of the early years of Canada's foreign service. In carrying out his task, the author chose to focus, somewhat narrowly perhaps but probably wisely, on Pearson himself, leaving unasked and unanswered many questions about Pearson's influence on the actual functioning of the early diplomatic service. He

deals with Pearson's relationship with the leading players, Canadian and foreign, and with the large events of the time, including the drift into World War II, the war years, and the early history of the United Nations.

Within these arguably astute parameters, this is a model biography and one well suited to the character and career of its subject. Breezy and informal, like Pearson himself (who is referred to throughout by his nickname "Mike"), it is an entertaining and at times gripping account, characterized by bold judgments, astute observations, and numerous insights into the Pearsonian persona. There is nothing much here to change the basic contours of our understanding of the man; we always knew that behind the appealing smile and the bow tie was a shrewd, perceptive, and, most of all, intensely ambitious man who craved success and approval. But by placing Pearson in his family background, a son of the manse whose childhood was spent in several small Ontario communities, all of them more or less the same, and by emphasizing the influence on the old Ontario of Protestant verities and British imperial values, English helps us to appreciate the style and the objectives of the later diplomat-politician.

At many points, too, English deals frankly and fully with critical turning points in the young Pearson's formative years and early career. Thus we learn of the bus accident that ended his World War I military career before he saw active service and of the shattered nerves—English calls him a "broken human being" (p. 52)—that affected him for some time after discharge. We learn of the distance he traveled early in life from his father's Methodist faith and of Pearson's own somewhat disheartening search for a career, which included false starts in the meat-packing business in Chicago and as a young lecturer in the history department at the University of Toronto who was profoundly unsuited to a life of a scholar. First, however, we accompany Pearson through his two years at Oxford, where he pursued sports, especially hockey, more avidly than his studies, deepened his anglophilia and confirmed his belief in those British values that English describes as "liberal imperialism" (p. 93).

Yet, as English is at pains to emphasize, Pearson's disillusionment with academic life was not the result of any lack of ability. In 1927, when he aspired to join the Department of External Affairs, he received the highest score of any candidate that year in the civil service exams. And at External Affairs he was a rising star from the beginning, even though he failed to share the suspicion of British designs that was so marked a feature of the policy of O. D. Skelton, long the dominating force in the department. After stints in Ottawa, London, and as a member of the Canadian delegation to the League of Nations, Pearson arrived in London in 1936 and served in the High Commissioner's office for most of the next decade. In London he developed his marvelous ability to win the confi-

dence and friendship of a diverse group of empire and foreign diplomats and to maintain the high regard of such polar opposites as Under-Secretary Skelton and High Commissioner Vincent Massey.

In 1943 he moved to Washington as the ambassador's second-in-command, and in 1945 he became ambassador himself. Although Pearson would later be regarded as one of the most pro-American of Canadian political leaders, English emphasizes that his own liberal and democratic sympathies did not preclude serious reservations about some of the excesses of the American experience. He argues as well that for Pearson the shift from London to Washington was fortunate in its coincidence with the shifting balance of world power, but that emotionally, for Pearson as for many English-speaking Canadians, the ties to the United States were far weaker than those of history and blood which had bound English-Canadians to Britain. This difference, he insists, is critical in understanding the distinction between Canada's pre-war ties to the empire and the Canadian-American alliance of the post-1945 period (p. 252). Nonetheless, for Pearson personally, his presence on the Washington stage was significant in terms of contacts made, both with American diplomats and journalists and with representatives of those countries critical in the establishment of the United Nations and its agencies. As these events unfolded, Pearson was a leader in the articulation of the concept of "functionalism" and the concomitant option of Canada as a "middle power" that was to loom so large in Canadian diplomacy for two decades and more after the end of the war (pp. 280–83).

In this first volume of a Pearson biography, English deals more than adequately with these serious topics, but like every successful biographer he keeps the ambitions, achievements, and personality of his subject always in the foreground. More than ever, Pearson appears as the quintessential Canadian (or perhaps Ontarian) of his times. As English moves in volume 2 to the political years, he will find this balance more difficult to achieve, but the insight and skill he demonstrates in this book suggest he will be equal to the task.

PETER OLIVER
York University
Toronto, Ontario

W. H. HEICK. *A Propensity to Protect: Butter, Margarine and the Rise of Urban Culture in Canada*. Waterloo, Ontario: Wilfrid Laurier University Press. 1991. Pp. viii, 229. \$45.00.

This is good old-fashioned history applied to a novel topic—how margarine became available as a substitute for butter in the Canadian market. It is good because of the quality of scholarship displayed: the research is comprehensive, documentation meticulous, approach unbiased. It seems rather old-fash-

ioned because it ignores theoretical and quantitative methods in economic and social history that one might expect to find applied to this kind of topic today. W. H. Heick tells the story of how Canadian dairy producers kept margarine from being sold in Canada for some sixty years and advances a thesis that purports to explain why this policy was eventually reversed.

In 1886 the Canadian government prohibited the manufacture or sale of margarine in Canada, ostensibly to protect Canadians from an inferior product, but also to support the growing domestic dairy industry. The ban was lifted in 1917 because of wartime circumstances but reinstated in 1922 and it continued in force until the courts ruled it ultra vires of parliament in 1949. Since then some provinces, notably Ontario (by far the country's most industrialized province) have retained petty restrictions designed to differentiate the product from butter.

Heick's thesis is that the banning and then freeing of margarine in Canada reflected the power of the dairy industry in the earlier period and the erosion of this power as Canada became more urban after World War II. This seems plausible, but none of the easily imagined alternative explanations are examined. Particularly disturbing is the author's handling of economic factors. In a chapter entitled "Economics of the Butter and Margarine Industries," Heick makes reference to the bizarre administrative system that has replaced the free market for dairy products in Canada. Canadian dairy farmers have been provided with government-sponsored, cartel-like marketing schemes that limit output and keep prices and producers' incomes higher than they otherwise would be. Yet the author suggests in this chapter and elsewhere that the dairy industry has been incapable of supplying the needs of consumers (p. 133) when it has clearly been able to produce a surplus. We are also told that both the butter and margarine industries contribute to the maintenance of a high standard of living for most Canadians. The reader who pays attention to the evidence presented about the organization of these industries may find this hard to swallow.

While it is surprising that a careful scholar would address the economics of an issue without making reference to the professional literature in the field, it is also odd to find not a single reference to the large literature on the workings of interest groups and the making of public policy in a study that seeks to explain, not simply describe, how an industrial lobby could succeed in denying consumers the benefits of choice over a period of more than half a century. Nevertheless, this volume does fill a gap in Canadian agricultural history and it documents an interesting case study in agricultural policy.

KENNETH J. REA
University of Toronto

LATIN AMERICA

GEORGE KUBLER. *Esthetic Recognition of Ancient Amerindian Art*. (Yale Publications in the History of Art.) New Haven: Yale University Press. 1991. Pp. xvii, 276. \$32.50.

The art and architecture of pre-Columbian America occasioned the first Europeans to see them with a mixture of admiration and puzzlement. Whether it was the buildings of Aztec Tenochtitlán and Inca Cuzco, or Amerindian treasures and manuscript codices that found their way to Europe, European beholders were at a loss as to how to understand their reactions. Beginning with these inchoate first impressions, George Kubler describes how the study of Amerindian art evolved, how this art gradually became aesthetically recognizable. The main body of the book consists of some seventy biblio-biographical studies of writers who lived between 1492 and 1984.

The list begins with some of the Spanish historians, missionaries, and *erudits* of the sixteenth and seventeenth centuries who laid the foundations for the study of Amerindian history, culture, religion, and art, and it ends with scholars who were Kubler's older contemporaries in Latin America, the United States, and Europe. The focus is on those who wrote about Central America, although some authors dealing principally with the Andes and North America are also included. The vision of the book is personal, even somewhat idiosyncratic, and is defined by some notable exclusions (for example, Francisco Javier Clavijero, author of the epoch-making *Storia antica del Messico* of 1780–81, and the scholar-traveler Ephraim George Squier, although both mentioned in passing, receive no entries) and inclusions, among them Charles Darwin and Karl Marx. The portraits constitute one of the strengths of the book; indeed, one would like to read more about the impact on Americanists of figures such as Vico, Herder, and the medievalist Viollet-le-Duc (pp. 9, 87–89, 106).

This work, however, is more than a repertory of writers on Amerindian art and cognate topics, for Kubler also propounds a thesis about the nature of aesthetic cognition and the aesthetic choices he considers to result from it (pp. 20 and following, 152, 177–78). Throughout the book, Kubler suggests that aesthetic choice lies at the root of cultural difference. One might say that in the task of explaining the particularity of Amerindian art, aesthetic choice is to Kubler what diffusionism and the tracing of successive waves of immigration into the American continent have been to his intellectual predecessors from the sixteenth century onward. Despite recurrent references to it, however, the nature of aesthetic choice, what exactly one can mean by it in a given context, and how it could be documented, are not fully articulated. In addition, the specific contributions to the aesthetic recognition of Amerindian art made by several of the earlier writers discussed here (one might mention in particular the descriptions by Ber-

nal Diaz del Castillo of Tenochtitlán and by Garcilaso de la Vega of Cuzco) are at times lost amid more general historiographic considerations. Even so, this is a thought-provoking and useful study that is rich both in erudition and insight. Copy editing and proofreading, however, leave something to be desired.

SABINE MACCORMACK
University of Michigan,
Ann Arbor

CAROLYN E. FICK. *The Making of Haiti: The Saint Domingue Revolution from Below*. Knoxville: University of Tennessee Press. 1990. Pp. xv, 355. Cloth \$44.95, paper \$21.50.

This is an important book for the history of Haiti and the Caribbean, for the analysis of slave societies, and for the comparative study of social revolutions. It opens with a fine synthesis of the evolution and structure of plantation society in Haiti before the revolution, including a perceptive and sensitive treatment of voodoo as the core of a slave cultural consciousness opposed to the beliefs of French masters. The work continues with a nuanced discussion of eighteenth-century slave resistance, emphasizing *maronnage* as evidence of a deep and widespread longing for freedom.

The analysis then turns to the complexities of the revolution. The conflicts among factions of the colonial French as well as between French and *affranchis* (free mulatto proprietors), as they all struggled to respond to revolutionary developments in France, are detailed. So, too, are shifting diplomatic and military conflicts. But throughout, the analysis focuses on the independent participation of slaves and maroons in the conflicts of 1791 to 1804. Fick uses materials from archives in Haiti, France, and the United States to demonstrate that the slave majority fought independently for liberation within revolutionary conflicts, following local leaders. They allied with others, whether French, mulatto, or black, only as it served their primary goal. Fick also reveals that every leader who aimed for general command, from the French emancipationist Léger Félicité Sonthonax to the black liberator Toussaint L'Ouverture, sooner or later sought to reestablish plantation production and to force the ex-slaves back into dependent labor service. Yet the slaves, who had claimed freedom in over a decade of insurrections, were equally persistent in demanding access to land for subsistence autonomy. The revolution developed as a complex sequence of alliances and conflicts among those who would build a new state—and thus demanded production of a surplus—and those who fought for liberty and subsistence autonomy. The result was a Haitian nation based on a peasant economy built adamantly by ex-slaves, but ruled by autocrats who

would squeeze whatever surplus they could from families of subsistence producers.

The book thus offers a persuasive explanation of the origins in revolution of the structural contradictions that have defined Haitian national history ever since. The exploration of popular participation in the revolution demonstrates, once again, the contradictions inherent in the relations between revolutionary elites and those they aim to liberate. Even Toussaint L'Ouverture, the leader most committed to slave emancipation and black equality, but facing powerful opposition to his project of Haitian independence, worked to rebuild the plantation economy so abhorrent to his followers. The resulting conflict dividing revolutionary leaders and followers decisively undermined the liberator's power—and demonstrated the independent role of the insurgent masses in revolutionary confrontations. These are the classic difficulties of revolutionary consolidation. New rulers come to power promising to relieve the burdens of an aggrieved people. But to consolidate a new state, they face powerful, hostile forces within and without. To resist, revolutionary elites build a strong state and impose new burdens of production and surplus extraction on people who had fought the revolution to escape such demands. In Haiti, as elsewhere, postrevolutionary reconstructions have been laden with contradictions between victorious leaders and popular insurgents.

Fick also explores with skill Eugene Genovese's argument that the Haitian uprising was the first modern slave insurrection; that it was the first such movement to seek a new future rather than the reinstitution of past justice. Her analysis shows that the slaves followed deeply traditional values in escalating *maronnage* into emancipation. Equally traditional was their family production in kitchen gardens, which they developed into the vision of a peasant subsistence economy. The old desire to escape slavery, thrust into the context of the French Revolution, became the progressive goal of national independence. Yet the maintenance of a nation-state, essential to emancipation, was inhibited by demands for a subsistence economy. Slaves who became revolutionaries were inherently both traditional and modern. Or perhaps our categories meant nothing to them.

The slaves-becoming-citizens who built Haiti fought for their own vision of the good life. They faced powerful outsiders who would reenslave them, and national leaders who promised to free them yet who nevertheless worked to exploit them in new systems of coerced labor. In the face of such pressures, the Haitian people built their peasant economy but never escaped the plague of coercive rulers. To those who had lived brutalized as slaves, it mattered not whether the goals of freedom and subsistence autonomy were traditional or modern. They fought simultaneously to escape old burdens and to build a new society free of such impositions. They claimed

important, but partial, successes—freedom and national independence—that were enough to inspire slaves and terrify slaveowners from the Chesapeake to Brazil. The limits of their success, a tradition of coercion imposed by those who fought to rule them, would plague their descendants for generations.

JOHN TUTINO
Boston College

LARRY R. JENSEN. *Children of Colonial Despotism: Press, Politics, and Culture in Cuba, 1790–1840*. Tampa: University of South Florida Press. 1988. Pp. xix, 211. \$22.00.

So often neglected in the study of late colonial Latin American history, the periodical press in each country represents a treasure-trove of political, cultural, and sometimes economic comment. This is ably demonstrated in Larry R. Jensen's work on Cuba.

This fifty-year span was an era of remarkable, but largely unstudied, change and struggle in Cuba, a time when Spain's other New World colonies were breaking asunder their mutual ties. Why did the same not occur in Cuba? One reason was certainly the heavy hand Spain imposed on the press, together with creole acceptance of these restrictions. Still, a press continued to exist through these years, and a relatively free press surfaced in three brief, three-year intervals when constitutionalism held sway: 1811–14, 1820–23, and 1836–39. Jensen's fascinating and ably written volume plumbs these years particularly, utilizing several extremely complete press collections—weeklies, biweeklies, even a few dailies, although not newspapers in the modern sense. Some were little more than flyers, others appeared as crude magazines. Much of this material is now in the archives in Seville. Additionally, there have been several Cuban-published studies based on limited research into these materials. Jensen's research goes far beyond these earlier writings.

What emerges is a careful analysis of these critical years in the twilight of Spain's colonial era on the Caribbean island as the officialdom and its creole supporters sought to stem the inevitable tide of independence. This study serves, in part, as a valuable companion for Cuban historian Manuel Moreno Fraginals's pioneering three-volume work *El Ingenio* (1978) in the same period of Cuban history and focused on sugar and the sugar oligarchy.

Although the Spanish monarchy in 1824 had conferred on Cuba the title "siempre fiel" (the "ever-faithful" island), the seeds of rebellion were ever present, but often submerged. Spanish officialdom was working desperately to hold on to the island, worried that it would follow the larger and richer mainland colonies into independence with the consequent loss of the not inconsiderable Cuban revenues. Much of the Spanish effort was directly conciliating the sizeable sugar oligarchy and its supporters. For a

time the approach worked, as Moreno Fraginals has shown and as Jensen shows here. But the steady clamor of the Havana literati, often supported by growing dissent among the sugarocracy, for greater freedom of expression in writing and in other cultural forms was to steadily chip away at ties with Spain throughout the early years of the nineteenth century. The press, according to Jensen, was clearly in the vanguard of this movement. Words were important—sometimes inflammatory, often a challenge to thought and conscience.

For Jensen, the moments of censorship are often as important to study as the years of relative press freedom. He argues that such study lifts the veil on the culture of the time and shows why colonial society often rejected new ideas and went along with Spanish authority. There was concern, for example, among some of the creole literati about romanticism as expressed in Wordsworth, Byron, and Scott. In 1814, an article in *Noticioso* (which had a longer life span than most papers, existing from 1813 to 1844) "denounced the imaginative excesses of romanticism" (p. 103), as Jensen writes. Yet, ten years later, romanticism was in full sway on the island, albeit with able literary detractors. Jensen uses these changes in colonial thinking to show how the mood of the Cuban people, as reflected in the colonial press, flip-flopped in support of, or rejection of, Spanish authority.

For researchers who might question the accuracy of the press, who doubt the wisdom of relying on the press for a look at society, I would agree that journalism—both yesterday and today—is far from an exact science. The press, however, is often a barometer of where society is at a given moment. The press, in short, can be properly used to support other archival research, to measure that research against the reality of public thought on issues, even in times of great censorship. This is precisely what Jensen has done and is the value of his able study.

JAMES NELSON GOODSSELL
The Christian Science Monitor
Boston, Massachusetts

BIRGIT SONESSON. *La Real Hacienda en Puerto Rico: Administración, política, y grupos de presión (1815–1868)*. (Economía quinto centenario, monografías.) Madrid: Instituto de Cooperación Iberoamericana, Sociedad Estatal Quinto Centenario, and Instituto de Estudios Fiscales. 1990. Pp. 418.

Andrés Pedro Ledru, a French scientist who visited Puerto Rico in 1797, just prior to the period covered by the book under review, observed: "Public administration in Puerto Rico is so enshrouded with shadows and mysteries that a foreigner can barely penetrate them." Birgit Sonesson, after extensive research, has managed to bring light to some areas of the subject, but several mysteries still remain to be illuminated. The bulk of the research was done in Spanish

and Puerto Rican archives, and only selected references are made to material in the Danish and French archives where the author has done work for previous studies. There are few references to U.S. documents or archives.

The narrow focus of the work is indicated in its title and subtitle, but the study is basic to any attempt to examine the trade, commerce, and economic development of the island in the first half of the nineteenth century. After a description of the colonial fiscal administration resulting from Spain's weak attempt to reward the loyalty of its remaining colonies, the author explores the orientation offered by the public servants named to fill the newly created (for Puerto Rico) post of *intendente*, or what we might call today an economic planning official. The second area Sonesson studies is more elusive, since Puerto Rico was deprived of any opportunity to participate in political decisions taken in Madrid.

Sonesson deserves credit for the identification and study of pressure groups. She goes beyond the labels of progressive, moderate, free trader, and conservative monarchist to identify the professional or the businessman: exporter and importer, the landowner producing sugar, coffee, and cotton. She makes a cogent case for ascribing to them opinions on fiscal, trade, and economic policies.

From this careful and detailed study it would be one small step to answer the question as to why Cuba was in frequent rebellion against Spain in the nineteenth century while Puerto Rico was relatively passive. The demonstrated trade flowing through the nearby Danish port of Charlotte Amalie provides one possible answer.

Sonesson's dissertation, on which this publication is based, merited two prizes in 1987 granted by the 500th Anniversary Commission of the Discovery of America, one award for the best work on Spain and the Americas and the other for the best thesis in English on the history of Spain and Portugal.

THOMAS MATHEWS
Association of Caribbean Universities
San Juan, Puerto Rico

SAMUEL Z. STONE. *The Heritage of the Conquistadors: Ruling Classes in Central America from the Conquest to the Sandinistas*. Foreword by RICHARD E. GREENLEAF. Lincoln: University of Nebraska Press. 1990. Pp. xiv, 241. \$35.00.

Samuel Z. Stone, whose previous major contributions to Central American history have been in the field of genealogy, has now broadened his concern to examine the legacy of the conquistadors. He concludes that their descendants still rule and impose their culture and values on the five nations that make up Central America. In itself such judgment should surprise few Central Americanists: what is astonishing is the extent of the consanguinity that this extraordinary research

reveals. For example, Central American presidents in the five different states are descendants of one conquistador. (Anastasio Somoza García and Oscar Arias Sánchez were related, and many Sandinistas have relatives among the Contras.) Another conqueror had bloodlines linking him to sixty-one Costa Rican congressmen.

Having uncovered all this fascinating information, the author faces the matter of significance. He finds that the old colonial aristocracy left an important imprint on Central American society that is still visible today. Furthermore, historians, always preoccupied with foreign influences on the isthmus, have dangerously ignored the culture and values of these founders. One should not quarrel with Stone's warning that Central Americans must find Central American solutions to their problems, but I am not yet ready to subordinate the consequences of decades of British or North American hegemony, vast foreign investment and immigration, military intervention, canal schemes, and so on.

Stone's book is essentially a group of lightly related essays fostering subthemes of varying importance. One such subtheme attempts to delineate a north versus south differential in Central America, a useful tool at times. But it is not one that reconciles itself with the fact that Central American nations are different but also alike, a point probably first reported by Dana Gardner Munro. To make his case Stone classifies the south as Nicaragua and Costa Rica, Costa Rica alone, or even those two plus Honduras. The material will not stand still for too much taxonomy.

In another forum, time or space might be available to debate other conclusions, such as the contention that a scarcity of economic resources spurs democracy (p. 131) or that the elimination of Costa Rica's army directed its citizens down the path of a doomed welfare state (p. 107). Stone's attitude toward Costa Rica is refreshing; he acknowledges its usual claim to superiority over its neighbors but is frightened by its present welfare state and seems to pine for the "good old days" before José Figueres, when coffee was king.

The brief essays are filled out with little tales, fanciful and interesting. The writing is clear and technically error-free. The bibliography contains much that is traditional, augmented by the author's research in genealogy. But top-notch biographies such as those by Charles Ameringer, Kenneth Grieb, Neill Macaulay, and Stephen Webre, to cite but a few, are ignored, as are standard diplomatic and economic studies. The appendices contain genealogies worth the price of the book.

THOMAS L. KARNES
EMERITUS
Arizona State University

MARIO SAMPER. *Generations of Settlers: Rural Households and Markets on the Costa Rican Frontier, 1850-1935*.

(Dellplain Latin American Studies, number 26.) Boulder, Colo.: Westview. 1990. Pp. xvii, 286. \$32.50.

Most Latin Americanists associate the growth of export agriculture with the expansion of large estates. Recently, however, there has been a growing interest in the vital role that peasant farmers have played in producing export crops in some regions. This book by Mario Samper places the Costa Rican experience with small coffee farms in comparative perspective, thus contributing to our understanding of variations in the growth of agricultural capitalism and the continuing viability of peasant production in Latin America today.

One of the new generation of Costa Rican socio-economic historians, Samper traces the history of the northwestern part of the Central Valley of Costa Rica from its initial settlement in the early nineteenth century through the 1930s, when the frontier had effectively closed. He explores the strategies that peasant households used in their struggle to survive as the population grew and land became increasingly scarce.

This book has many strengths. It is clearly conceptualized; the theoretical introduction and the conclusion should be stimulating reading for anyone interested in studying household production and peasant decision making over time. The case study of the northwestern Central Valley adds to our understanding of frontier expansion in Latin America by explicating settlers' motives for migration and by emphasizing their commercial orientation. Also, Samper sheds new light on the impact of coffee on rural society by examining coffee as but one part of a complex rural economy. Coffee in nineteenth-century Costa Rica did not displace other crops: rather, the introduction of coffee gave rise to a highly diversified and increasingly commercialized economy in which small and large farmers raised corn, beans, sugar, coffee, and cattle for sale. Samper clearly describes the economic bases of wealth and rank that structured rural society and how these bases altered as the population grew, competition for land increased, and production became more market oriented. The author also explores the problem that partible inheritance posed for farm families and the various strategies they adopted to keep their meager patrimonies intact. The book contains stimulating observations on changing inheritance patterns, the functions of credit, and the logic of land use. And Samper's interviews with old people provide fascinating glimpses of peasant economic morality in the 1930s. This case study is based on the innovative and admirably thorough use of probate inventories, agricultural censuses, and municipal, judicial, and parish records.

The author's relentless insistence on using quantitative archival sources is both a strength and a weakness. Whereas his microexploration of household decision making is certainly insightful, Samper ne-

glects to elucidate the larger economic, social, and political context, perhaps assuming that his readers are familiar with the more broadly based studies of Carolyn Hall and Lowell Gudmundson. Despite its title, this book reveals little about markets on the Costa Rican frontier. Virtually no information is presented on infrastructure, prices, markets, urban growth, or changing consumption patterns. Furthermore, although he maintains that political power shapes rural society, Samper says almost nothing about government policy or the relation of small farmers to coffee processors and merchants. One wishes that he had been willing to range beyond his very focused sources to give the reader more sense of context and more interpretation of the implications of his findings.

In sum, this is a meaty, though sometimes dry and frustrating, book. It has much to say both theoretically and empirically about peasant household strategies under evolving economic conditions, and it impresses the reader with the extraordinary complexity of rural life. Like a number of recent studies, this book aims to counter the myth that Costa Rica was an egalitarian rural democracy of subsistence-oriented peasants. The final chapter, which explicitly compares Costa Rica to regions of smallholder coffee production in Colombia, Puerto Rico, and elsewhere, does well in situating the Costa Rican experience cross-nationally.

CATHERINE LEGRAND
McGill University

JEFFREY L. GOULD. *To Lead as Equals: Rural Protest and Political Consciousness in Chinandega, Nicaragua, 1912-1979*. Chapel Hill: University of North Carolina Press. 1990. Pp. xi, 377. Cloth \$47.50, paper \$14.95.

In this fine work, Jeffrey L. Gould carefully details three phases of the struggles for labor rights and land by Nicaraguan *campesinos* in the department of Chinandega. It is essential reading for anyone interested in Third World peasant or labor movements, and it makes a valuable contribution to understanding the origins of the Nicaraguan revolution, and to understanding the Somoza dynasty and its relations with both upper and working-class social forces.

The book has three sections. Part 1 examines labor and politics from 1912 to 1949 and describes the background and development of the great San Antonio sugar mill and its labor relations (1890-1938), then follows the labor movement there from 1944 to 1949. There is also a chapter on the workers' movement of the city of Chinandega. Part 2 details the relationship of Chinandegan peasants seeking land to cultivate with the Somoza regime during one of its populist phases from 1950 to 1964. Enclosure of land once available to peasants by largeholders led to considerable peasant mobilization. Leadership of the peasant movement in various conflicts over land and

access to land often sought to engage the Somoza regime and Liberal Nationalist Party as allies, but met with only modest success. This section reveals the contradictions and strains in the Somocista state and politics through detailed treatment of the intricate maneuvers between one or another Somoza, unions, peasant leaders, peasant followers, parties, and party factions. Chapter 10 provides a fascinating picture of the roles of peasant women in labor and land politics.

Part 3 explores the links between Chinandegan peasants and the Sandinista revolutionary movement between 1964 and 1979. Chapter 11 looks at agrarian reform in the 1960s and early 1970s. Chapter 12 traces the contributions of labor and peasant politics and radical Christianity to the rise of peasant insurgency and to the success of the Sandinista National Liberation Front in the late 1970s.

Gould amply and to excellent purpose employs primary sources (public and private documents and many interviews with eyewitnesses and participants). He has achieved remarkable levels of access to the participants on many sides of the various struggles he documents. The book is well written and engaging. He is at his best in laying out the complexity of peasant-landlord-state-politico relations and worker-union-firm-state-politico relations. Gould here contributes significantly to our knowledge of how the need for land or some other economic necessity becomes actual political mobilization. While I believe the book would be hard going for undergraduates, it is a must for graduate students and scholars interested in Nicaraguan history and politics. By presenting the thinking of participants in such struggles at a microanalytical level, Gould complements exceedingly well several recent macro-level studies of how agrarian economic forces have led to political unrest in Central America.

JOHN A. BOOTH
University of North Texas

SERGE GRUZINSKI. *Man-Gods in the Mexican Highlands: Indian Power and Colonial Society, 1520–1800*. Translated by EILEEN CORRIGAN. Stanford: Stanford University Press. 1989. Pp. 223. \$32.50.

On the eve of the quincentenary of the "encounter" of European and Native American cultures, as we gear up to hear more tales of Columbus, Cortés, and Moctezuma, we also need to be reminded, many times over, that the search for redemption from the spiritual conquest has been a shifting, centuries-long process involving many unsung historical players. Moreover, that search has not yet ended. Serge Gruzinski's book is a profound meditation on the spiritual conquest of central Mexico and its Christianization of the local religious imagination. Embracing the entire colonial period and daring even to forge speculative connections to the postcolonial aftermath, Gruzinski offers the kind of broadness of vision that is

so often lacking in studies of the religious imagination in Mexico. One comes away from the book with a wide-angle picture of the conquest and its repercussions within the diffuse, but highly significant, realm of spiritual power.

Gruzinski takes the reader beyond the canonical early "encounter" of Spaniard and Indian to uncharted historical territory, focusing on the challenges posed to colonial spiritual authority by four Indian mystics who claimed Christian religious power as "man-gods" and sought to lead cults of their own. The book covers three centuries and revolves around the stories of four remarkable men who, like Menocchio of Carlo Ginzburg's *The Cheese and the Worms*, were highly creative cosmologists who came into conflict with the established church. Gruzinski acknowledges that the "man-gods" of his book were "a statistical rarity," but it is their singularity, he believes, that makes them interesting, because they represent "an incomparable attempt to seize and master from below the change that affects and crushes them" (p. 181). The first, Andrés Mexcoatl, appeared before the Inquisition in 1537, during the early stage of Franciscan mission building, after calling for a return to the worship of the old gods. The second, Gregorio Juan, claimed in 1659 to be the Son of God and, already highly acculturated to the mythologies and prejudices of Christianity, envisioned a syncretic cult in which both Jews and Judas empowered him to act with the wrath of God. The third, Juan Coat, dressed as a priest, heard confessions, and baptized and married his followers at a mountain shrine in 1665. The fourth, Antonio Pérez, likewise played the part of a priest, telling his followers in 1761 that he himself was God and staging healing rituals in which the images of Christ and the Blessed Mother assumed miraculous powers.

The fifth character in the story, who is only mentioned in the last four paragraphs of the book's conclusion and therefore remains shadowy, is none other than Emiliano Zapata, the revolutionary leader who disappeared mysteriously and has yet to return to carry out his promise that "the land belongs to the one who works it." Gruzinski asks whether he might be "the last man-god," leaving open the possibility and suggesting that the road is now clear for ethnographers to embark on the task of telling the stories of contemporary man-gods who continue to live on in spiritist cults and popular legends. As an ethnographer myself, I would reply to Gruzinski that while I have not come across the view of Zapata as a man-god, I have, in the course of fieldwork in the more northern region of San Luis Potosí, encountered a spiritist cult that treats Pancho Villa as just such a man-god, who appears in the flesh during monthly healing sessions led by a female spirit medium who acts as his incarnation.

Perhaps Gruzinski's most brilliant insight is his assertion that, in the dialectics of colonial domination and resistance, there were several modalities available

to colonial subjects for action, power, and syncretic elaboration. Ironically for the colonizers, religious acculturation often took place with a vengeance, such that "the Indians could accept Christianity to the point of taking possession of God and his sanctuary" (p. 87). Indeed, a range of positions was being defined and contested during the struggle for the church that took place from the period of the Franciscan missions to the baroque age. While Gruzinski resists making comparisons with messianic cults of the Andes, Africa, Melanesia, and medieval Europe because of his distaste for typologies, he might have been well served by placing his study within the context of recent work on the conflicts between priests and their parishioners in traditionally Catholic regions. Many features of the dynamics of the struggle for the church that Gruzinski analyzes in the Mexican colonial setting from an ethnopsychiatric perspective could be fruitfully understood in terms of the tension between orthodoxy and popular religion, at least certainly by the seventeenth century.

A reader with any rudimentary sort of gender consciousness will also want to know where to find the women in the vast panorama of religious elaboration and revitalization that Gruzinski evokes. This book is decidedly about man-gods. Were there no woman-gods in colonial Mexico? Gruzinski does not address this question. Yet there is evidence in criminal and inquisitorial documents that women, too, served as shamanic leaders, offering highly contradictory visions of a new world liberated of Christian hegemony and yet saturated with Catholic redemptive thinking. Gruzinski's failure to conceive of the colonial religious scene in gendered terms results in an incomplete view of the spiritual conquest as a site of contestation that mainly involved male European clerics and male Indian cosmologists. The book is based on the assumption that its subject is "the Indian mind" (p. 50) when, in fact, it is about the contest for male redemptive power that male colonized subjects sought to wage, at once bravely and hopelessly, against male religious colonizers. Had gender been one of Gruzinski's analytical tools he might have been able to go further in his interpretation of the configurations of Indian power and resistance.

Even with these gaps, Gruzinski's work is uniquely creative and important in the field of Mexican cultural studies. His many insights into the way European colonial power was transfigured by the local religious imagination are unsurpassed in the literature on the spiritual conquest of Mexico. In a field where the early period of the conquest and the contemporary postcolonial moment are so overdocumented, it is refreshing to learn in such striking detail of the crucial middle years of colonial domination and resistance. Gruzinski has built a strong intellectual bridge that connects the territories of the past and the present, a bridge that deserves to be traveled by social historians and cultural anthropologists interested in a fresh reading of the texts of Spanish

colonial rule. In the margins of those texts, as Gruzinski reveals, one can see the signs of a native religious discourse that blossomed in the midst of colonial domination, a discourse that questioned the very basis of European spiritual authority, and that, once set in motion, could not be subdued or undone, even by those who so carefully recorded its challenges only in hopes of erasing it.

RUTH BEHAR
University of Michigan,
Ann Arbor

DOUGLAS MONROY. *Thrown among Strangers: The Making of Mexican Culture in Frontier California*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xviii, 337. \$29.95.

The story of the making of Mexican culture in California begins with the area's Native Americans, their economic activities, and their autonomy as nomads. Intent on restricting that freedom, missionaries and soldier-settlers spearheaded the colony's expansion into the region in the 1760s and, with a variety of enticements and force, "reduced" practically all the Indians into mission towns by 1828. But Spanish attempts at the social-cultural transformation of the natives elicited wide-scale resistance, defections, and revolts. In the end, however, the introduction of European diseases, new diets, physical and emotional strain, and, under the Americans, warfare, took its toll on the Indians, whose numbers dwindled from close to 150,000 to fewer than 20,000 by 1850.

In the meantime civilian settlers who established towns such as Los Angeles (1781) challenged the padres' claims to the good lands and to Indian labor. While the settlers had only limited success at this, they gained considerable economic strength by means of an active Yankee clipper ship trade, in which they exchanged a variety of livestock products for manufactured goods. The settlers also acquired, in the 1820s, a mighty weapon in the liberal ideas of the Enlightenment that enshrined individualism. With their new wealth and the liberal ideology the *pobladores* battered down the mission system by the end of the decade.

But the economic forces that empowered the Californios quickly overwhelmed them as well. American merchants and settlers, backed by a stronger economic system and arriving in increasingly larger numbers, swept aside the *rancheros*, despite their attempts to stem this tide. In the midst of new exploitative and depersonalizing forces, Mexican Americans struggled mightily—and often succeeded—in revitalizing their communities that had been all but destroyed by the mid-century economic and demographic storm.

Douglas Monroy presents us with a good synthesis on frontier California that is in keeping with the new

socioeconomic trends in borderlands history. But this book has some serious shortcomings. While countering the triumphalist pro-mission, pro-Spanish, and pro-American histories of the past, he presents a California peopled by an unsavory lot. This picture results from the author's narrow pro-Indian perspective and his acceptance at face value of statements made by competitors who, in vying over resources, maligned one another relentlessly. Although these views are qualified at times, they are repeated so often that the negative image nevertheless remains. If the Spanish-Mexican culture perdured into the American period, one might wonder, from Monroy's study, if it was necessarily a good thing that it did.

More importantly, the author fails to provide a full understanding of the dynamics of the formation and development of the communities that survived the changes in sovereignty and became the beacon lights for later immigrants from Mexico and the anchors for Mexican-American identity in the present. Still, as the best single work on this topic to date, this book does shed some light on these processes and the resilience of the Spanish-Indian-Mexicano culture.

GILBERTO M. HINOJOSA
University of Texas,
San Antonio

HAROLD DANA SIMS. *The Expulsion of Mexico's Spaniards, 1821–1836*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1990. Pp. xii, 277. \$42.00.

Historians have gradually filled in some of the glaring gaps in our knowledge of the period between Independence and the Porfiriato in Mexico. Recently, the researches of Barbara Tenenbaum, Donald F. Stevens, Stanley Green, Jan Bazant, David Walker, Michael Costeloe, and Harold Dana Sims have enriched our understanding of different facets of economy and politics.

Sims has researched and written for two decades on the expulsion of Spaniards from Mexico in the early independence era. The book under review is his fourth volume on the topic, the first three of which were published in Spanish in Mexico, and it is intended to summarize and clarify his earlier findings and to extend their chronological scope to include an evaluation of the effects of the last expulsion order. There were three different expulsion orders: 1827–28, 1829, and 1833–34. The author maintains that they were “relentless . . . and never wholly successful” (p. 6). But they virtually eliminated Spaniards from the military, the bureaucracy, the mining industry, and the church. The expulsions played out as the crucial element in the rivalry between *Yorkinos* (York rite masons), who favored elimination of the Spaniards, and *Escoceses* (Scottish rite masons), who opposed it, for control of Mexican politics.

The first expulsion, in 1827–28, failed, first be-

cause it exiled only 27 percent of the *peninsulares*, and second because the exiles and their brethren who remained in Mexico provided substantial support for reconquest attempts by Spain. Most important, the fear of Spanish invasion caused the Mexican government to bankrupt itself by expending 80 percent of its budget on the military. The expulsion orders, moreover, obligated the national government both to pay for the expenses of the exiles and to continue to pay their salaries, if they were soldiers or government employees, which added further to the nation's fiscal woes.

The second expulsion, in 1828, created an enormous drain of capital. Foreign trade, once in the hands of Spanish merchants, dropped. Because government revenues relied almost entirely on levies on external commerce, Mexico was dealt a further blow.

The third expulsion, in 1833, had the least effect. Spain's belated recognition of Mexican independence in late 1836 and the conservative's ascendance after the liberal debacles of 1833 ended the push for elimination of the Spaniards.

All of the expulsions stimulated a lucrative commerce in circumventing them. A system of “private intrigue and negotiation” arose (p. 96). This reached such an extent that conservatives charged that the anti-Spanish laws were a form of speculation.

The desire to eliminate the Spaniards from Mexico was, according to Sims, a popular sentiment that arose from the fear of reconquest. But the expulsions helped to substantially undermine the new nation's fiscal stability, thereby endangering it far more than the continued presence of the hated *gachupines*. Sims's detailed examination provides us with an important piece of the difficult puzzle that comprises the history of early Independence-era Mexico.

MARK WASSERMAN
Rutgers University,
New Brunswick

PAUL GOOTENBERG. *Between Silver and Guano: Commercial Policy and the State in Postindependence Peru*. Princeton: Princeton University Press. 1989. Pp. x, 234. \$39.50.

Paul Gootenberg's book is the first monograph in English devoted to the period from independence to the 1850s in Peru. It explains Peruvian political and economic development at that time in ways that will be useful to a variety of different scholars.

For those concerned with historical theory, Gootenberg presents valuable insights by using the Peruvian situation as a case study in “dependency theory” to test whether it provides an adequate explanation for the period from 1824 to 1854. As his work carefully demonstrates, the strange consummation of independence in Peru, where foreigners such as Simon Bolívar won the war against Spain, brought to power varieties of xenophobic nationalists determined to

enforce protectionism instead of the kind of free-trade regimes dominated by relations with more advanced economies that dependency theorists would and have predicted. Only with the bankruptcy of both the ideology of protectionism and its merchant backers and the subsequent miraculous appearance of a fabulous export commodity, guano (dried bird droppings used for fertilizer), was Peru able to form a state that could then be linked to the world economy. Indeed, Gootenberg argues that Peru had to develop such a genuine nation-state for it to join the world economic order so that it could become dependent on it.

For historians of countries loosely referred to as belonging to the Second World and Third World in general and to Latin America in particular during the "modern" period, Gootenberg recounts a Peru replete with fascinating comparative possibilities. The country that emerges from this wealth of primary evidence thoughtfully analyzed and synthesized looks superficially similar to some of the other republics in the region (and to other areas of the world no doubt), and yet is wildly different. For example, Mexico suffered ostensibly comparable instability for the same period (and even longer), but it managed to construct a much more elaborate tax structure and at least until 1856 avoid the kind of economic disaster on both the fiscal and economic level that Peru suffered on the eve of guano prosperity. Gootenberg's portrayal of Peru hopefully will stimulate the necessary reevaluation of how other countries managed the difficulties following independence and the true range of options available to them.

Furthermore, this book is a substantial addition to Peruvian history and demonstrates, yet again, the importance of fiscal issues in determining the fate of a nation. It places the constant caudillo wars in their economic context, offering a systematic guide to the intricacies of national finance as well as offering the first cogent explanation of Agustín Gamarra, the military leader whose death paved the way to Ramón Castilla and the development of the Peruvian state.

Finally, Gootenberg details a complex period in a beautiful and often witty prose. His book sets a new standard for the analysis of post-independence Latin America and makes a valuable contribution to the field.

BARBARA A. TENENBAUM
Encyclopedia of Latin American History

WINTHROP R. WRIGHT. *Café con leche: Race, Class, and National Image in Venezuela*. Austin: University of Texas Press. Pp. xi, 167. \$25.00.

Surprisingly little has been written on race relations in modern Spanish America. Winthrop R. Wright has made an important contribution toward filling this lacuna with his research on Venezuela. Having plowed through the press, congressional debates,

scholarly writings, essays, and fiction of the last century, he skillfully traces the elite preoccupation with the racial character of the society it controlled.

What emerges is a clear picture of an insecure ruling oligarchy that in the second half of the nineteenth century struck its compromise with the "scientific racism" penetrating Latin America from the intellectual centers of North Atlantic civilization. Unable to refute racist dogmas, Venezuelan writers and politicians chose, like their counterparts in Brazil, to turn those dogmas on their head. They acknowledged that Venezuela was a racially mixed society, although clinging to the fiction that the non-white component was strictly Indian (at least 60 percent was black or mulatto by the early nineteenth century). They enthusiastically endorsed miscegenation (more often in rhetoric than reality), arguing that it was effortlessly creating a whiter and, therefore, a better Venezuela. They thus had the best of both worlds: they could dissociate themselves from the violent repression typical of U.S. race relations (an evil that all Latin Americans scorned), and at the same time reassure themselves that they were rapidly approximating the "superior" race.

The logical corollary of this faith was a commitment to promoting white immigration, and the barring of all new non-whites. This policy was often reiterated from the late nineteenth century through the 1950s. Ironically, it was frequently violated in practice, as cheap black labor was imported from the Antilles, much to the disgust and fury of the champions of "whitening."

What is especially noteworthy in Venezuela is the manner in which a new political party, Acción Democrática, in the mid-1940s chose to challenge the prevailing elitist racial ideology, appealing (although not explicitly) to the racially mixed majority through a populist program aimed at promoting greater social mobility. Wright argues that as a result the small black population gained greater acceptance, although most remain trapped in poverty.

How special was the Venezuelan case? Wright suggests that Venezuela is exceptional in its lack of prejudice and discrimination against persons of color, ranking at the other end from Argentina, with Brazil located midway between the two extremes. Yet, given the lack of quantifiable data in both Argentina and Venezuela (unlike Brazil, where recent census data clearly document discrimination in earnings, education, and so on), such a judgment can be based on little more than anecdotal evidence.

Nonetheless, such evidence suggests that Venezuela enjoyed a striking degree of tolerance for mixed bloods, although prejudice and discrimination against blacks remains marked, by the author's own evidence. What is undeniable is that the Venezuelan elite, like their Brazilian counterpart, adapted racist doctrines to their multiracial reality, proclaiming the inevitability of an eventually white society. In the meantime, they congratulated themselves on their

success in having created a "racial democracy" that contrasted so favorably with the ugly brutality of racial oppression in the materially more prosperous United States.

It must be noted, however, that this is a study primarily of elite attitudes, not of race relations. An examination of race relations based on official data is impossible at present, since quantitative data on race has not been collected since the abolition of slavery in 1854. Wright's treatment is further limited by being restricted to elite opinion, thus invalidating his claim that the "official doctrine of nondiscrimination and lack of prejudice have permeated all levels of Venezuelan thinking on the subject" (p. 127).

It remains for historians to research the experience of other Spanish American republics to see how their elites (and their nonelites) grappled with this common problem of reconciling racist dogma and multi-racial reality.

THOMAS E. SKIDMORE
Brown University

HILDA SABATO. *Agrarian Capitalism and the World Market: Buenos Aires in the Pastoral Age, 1840–1890*. Albuquerque: University of New Mexico Press. 1990. Pp. xiv, 316. \$50.00.

Several scholars in Argentina are actively reinterpreting their country's incorporation into the world-capitalist system. Although still employing neo-Marxian tools of analysis, like many *dependentistas* of the previous generation, this new group does not view the results of that incorporation as having been entirely negative. Hilda Sabato's book on the expansion of sheep raising on the Pampas of Buenos Aires province in the second half of the nineteenth century is a case in point.

In examining wills and notary documents in Argentina and commercial materials in France and England, Sabato has assembled an impressive amount of information about wool growing principally in the rural zone immediately south of the city of Buenos Aires. She devotes chapters to land tenure, labor, large and small rural enterprises, commercial networks, and rural financing. The chapter on labor is particularly revealing.

If Sabato's study has a recurring theme, it certainly pertains to complexity and variety. The expansion of sheep raising, as Sabato shows, did not increase the absolute concentration of land and resources into the hands of a few landowners but instead spread opportunity widely among nearly all segments of the social order. Her analysis shows that those landowners who combined sheep raising with farming, cattle raising, marketing, and financing were better able to survive the periodic international price depressions. Harkening to the work of Stuart Schwartz on Brazilian sugar and Marcos Palacios on Colombian coffee, Sabato shows that the elite's advantages did not prevent

independent warehousemen, country storekeepers, consignment brokers, small farmers, and land renters from sharing in the business, especially during boom times. Not even the British-built railways put the native oxcarters out of business, for thereafter the cartmen functioned as short-haulers of wool fleeces between *estancias* and rail stations. The entire productive system neither led to monopolization by the big landowners nor excluded opportunities for social mobility of some fortunate workers.

Sabato, however, does not paint the remarkable economic expansion of the late nineteenth century entirely in bright colors. While she acknowledges that—especially early on—the shepherds shared profits and were able to move ahead into ownership, there persisted a subtle social discrimination against native-born, mixed-blooded workers. The descendants of *gauchos* remained as wage peons and itinerant sheep shearers. Basque and Irish immigrants especially shared profits for herding sheep, received promotions as managers, and saved money to buy land. Also, the expansion of sheep raising as well as farming did diffuse landholding to a degree. But the economic expansion of the second half of the century did not appreciably reduce the social exclusivity of a class of elite families, which continued thereafter to enjoy economic and political advantages. In other words, the social system responded to cultural as well as economic determinants.

One might fault Sabato for not wishing to depart from the occasionally imprecise Marxian terminology and for engaging in some rather circular argumentation. There are some redundancies, unclear meanings, and inscrutabilities. The reader might also criticize her for concluding that "the fertility of the soil in the pampean region enabled Argentine staples to be produced at a cost lower than the international market prices, thus giving way to a transference of surplus value in the form of differential rent from buyer to seller" (p. 282), then refusing to say how this surplus was distributed. Nevertheless, with due regard for the empirical evidence, Sabato has explained one element of Argentina's remarkable economic achievement in the nineteenth century. She leaves to historians of later periods the puzzle of why the once-vibrant Argentine economy ultimately became sluggish and decrepit. That is exactly as it should be, for the fault of the *dependentistas* had been to see in every historical period the mutated economic genes of Argentina's mid-twentieth-century deformities.

JONATHAN C. BROWN
University of Texas,
Austin

DANIEL JAMES. *Resistance and Integration: Peronism and the Argentine Working Class, 1946–1976*. (Cambridge Latin American Studies, number 64.) New York: Cambridge University Press. 1988. Pp. viii, 303. \$42.50.

Daniel James offers us a fascinating exploration into the enigmas of Argentine Peronism as he disentangles that most perplexing phenomenon, the post-1955 Peronist labor movement. His work explores the role of the Argentine unions as they emerged to become the principal intermediaries and interlocutors between the Argentine armed forces and civil society in the post-1955 era.

The author first traces the foundations of Peronist labor in the 1944–55 period. Here he rejects the more traditional view that attributed the rise (and persistence) of Peronism to the corrupt, personalist manipulation of unsophisticated *criollo* workers, and instead concurs with much of the recent scholarship that frames the origin of working-class allegiance to Perón and Peronism in terms of the workers' rational, "instrumentalist" pragmatism. James goes further, however, in asserting that worker loyalty to Perón was more than simply a logical response to economic grievances and class exploitation; that loyalty was also rooted in the workers' embrace of Peronism's essential embodiment and articulation of a new working-class popular political culture. Peronism redefined the notion of citizenship "within a broader, ultimately social context" (p. 14) as it promised the working class active participation in the reorganization and redefinition of the nation's social and economic life. James maintains that it was this promise, as well as Perón's caudillo-like appeal to Argentine workers not as individuals but as members of a cohesive social force, that would underlie their sustained allegiance to Peronism in the post-1955 era.

When James turns to his main purpose (to trace the course of Peronism within the Argentine unions during the period 1955–73), he focuses on three crucial and related questions: first, what was the nature of the relationship between union leaders and rank-and-file members in the Peronist unions; second, how valid are the popular images of Argentine Peronist union violence, corruption, and politicking; and third, what were the real sources of the unions' and the leadership's power? James's intention is to "demythologize" post-1955 Argentine working-class history as well as to provide a corrective to a body of literature on recent Argentina that "fails to give us . . . any sense of the concrete historical experience of working people and their complex, ambiguous, frequently contradictory responses" (p. 3). And this he accomplishes exceptionally well. Indeed, one of the great merits of this study is that it probes beneath the vague and often facile "macro" generalities of conventional labor history. In this regard the work should serve as a model of historical analysis constructed from the bottom up, for it offers us an interpretation of recent Argentine union and working-class reality built on a nuanced and empathic understanding of the intricacies of Argentine workers' experience both on the shop floor and in the working-class barrio. In order to reconstruct and understand this working-class experience, James taps

(in addition to conventional archival sources) a vast array of unofficial Peronist and rank-and-file newspapers, shop-floor pamphlets, working-class barrio broadsheets, and the like, as well as an impressive assortment of interviews and conversations with all levels of participants in the events under review. Rather than cast the discussion of the increased bureaucratization (and indeed corruption) of mainstream Peronist unionism in terms of the moral failings of individual leaders such as Augusto Vandor, James argues instead from the vantage point of what he calls the "logic of institutional pragmatism" (pp. 130, 253). Without denying (and certainly without defending) the morally corrupt aspects of the mainstream Peronist union leadership, particularly *vandorismo*, James traces the emergence and dominance of union bureaucratism, with its heavy-handed and often violent control of rank-and-file challenges, to an implicit recognition among leaders and rank-and-file alike that the limited economic and political options in post-1959 Argentina demanded a *realpolitik* approach, a coherent, pragmatic, and compromising unionism. James convincingly argues that the acquiescence of the mass of Argentine workers to the dominant framework of Peronist unionism in the early and mid-1960s reflected and arose out of the concrete social, economic, and political experiences of workers in the wake of the defeat of the militant *resistencia* in 1959–60. The demoralization that followed, combined with the government's and employers' economic, legal, and not infrequent armed assaults on workers, undercut both the militancy and participation of the Peronist union rank and file.

James painstakingly deciphers and explains the complex competing strains of the post-1955 Peronist labor movement. He is particularly persuasive in his explanation of the emergence of the *clasista* union militancy that culminated in the *cordobazo* of 1969 as well as in his discussion of the emergence of the new Peronist Left that rose to challenge the mainstream Peronist union leadership in the late 1960s and early 1970s. (Curiously, he fails to discuss or even mention the seminal violent confrontation between these two Peronist groupings played out in June 1973 at Ezeiza airport at the very moment of Perón's return from exile.)

It should be noted that although the principal concern and focus of the work is labor and working-class history, James also offers insightful analysis of the evolution of post-1955 Argentine government social and economic policy. Especially interesting is his discussion of the contradictions imbedded in Arturo Frondizi's *desarrollista* ideology and policy. Overall, this work makes a major contribution to our understanding of Peronism and recent Argentine history.

DAVID TAMARIN

State University College of New York,
Geneseo

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

I am writing in two capacities to respond to the *AHR*'s number on the Modern Middle East [December 1991]—first as president of the Turkish Studies Association, second as section editor for "The Middle East since 1500" in the third edition of the *AHA Guide to Historical Literature*, now in preparation under the general editorship of Professor Mary Beth Norton of Cornell.

I am sure that most Middle Eastern historians were gratified, as I was, to see the AHA respond to members' concerns about the region by devoting a number of the *AHR* to it. The articles, I believe, will be appreciated by many, specialists and non-specialists alike.

I note with regret, however, that the number includes no article on Turkey. The Turkish Republic has historically emphasized its ties to the West much more than any other Islamic country has. Turkey played a major role in the Gulf crisis, as a member of NATO. Turkey stands out as one of the three largest and most influential states of the Middle East, and its economic development has given it manifold ties to the rest of that region as well as to Europe. By most indicators of political or economic development, Turkey has been, and continues to be, one of the most advanced countries in the Middle East. Most historians see these achievements as rooted in the heritage of the Ottoman empire, a legacy shared to varying degrees by most present states of the region. Turkish culture, moreover, has historically been one of the three most influential of the Islamic World. To embark on a study of modern Middle Eastern history but ignore the Turks is to indulge in presentist and reductionist distortions that vitiate the endeavor. The

growth of European integration and the collapse of the Soviet Union (where five republics have largely Turkish populations) also raise the larger prospect that not just the Turkish Republic but a larger Turkic World will soon be as familiar to us as the Arab World has been in recent years.

Scholars wishing to know more about the historical literature on the modern Middle East should realize, too, that the nine articles you have published deal only with selected topics. They omit some of the most highly developed fields, such as Ottoman and Turkish history, as well as others that are now particularly dynamic, such as women's history or the study of Islamic thought and activism. When the third edition of the *AHA Guide* appears, I believe AHA members will find in it broader-ranging evidence of a scholarly literature that compares favorably in quantity and quality with that on other major regions. The new *Guide* will also include sections on other related subjects, such as "The Islamic World to 1500," edited by Richard W. Bulliet of Columbia University. Historians seeking to know more about the Middle East will benefit from supplementing the December 1991 number of the *AHR* with the new *AHA Guide* at its publication, which is expected by January 1995.

CARTER V. FINDLEY
Ohio State University

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of *Religion and the Radical Republican Movement, 1860–1870* [*AHR*, 96 (October 1991): 1305–06], Donald Jones questioned the accuracy of the statement on the book jacket that the study "shows for the first time how church and state interacted on the slavery question" during the Civil War. To interact, there must be a reciprocal dialogue, which my book has in substantial amounts. More than 480 contacts or communications were documented between politicians and religious-affiliated citizens from grass-roots levels to the highest echelon in the churches. No other book comes close to detailing interaction of this magnitude. The title of Jones's

published dissertation, *The Sectional Crisis and Northern Methodism* (1979), appears to deal with political action, but it turns out to be a volume on published sermons, articles in Methodist quarterlies, and a few church records. Unfortunately, I eliminated references to Jones's book without being aware of it when I reduced the number of pages in my manuscript. Jones has written a good study of Methodist social ethics, but he uses no political manuscripts or political correspondence. In his book, the names of only five political figures are mentioned, once each, not counting Lincoln and Johnson. I used 132 political manuscript collections besides the correspondence of two missionary societies, the Freedmens Bureau Papers, and more than a dozen collection of papers of clergymen.

The excellent studies by George Marsden, James Moorhead, Martin Marty, C. Dunham, Ralph Morrow, and Michael Quill do not treat the interaction of politics and religion. Morrow's book has some eight items of correspondence between citizens and politicians, some of whom were ministers. Quill and Dunham used political papers sparingly, but the correspondence was not moral in tone or primarily from religious citizens.

Jones protests that "Howard shows no explicit awareness of a vast literature exploring precisely the subject matter of this book." Since he questions my understanding of social ethics and religion, I would refer him to my *Conscience and Slavery*, which was published in the same month. Since it deals more with religion than politics, it was placed in the context of religious historiography. Placing *Religion and the Radical Republicans* in the context of social ethics would have distorted my purpose. The proper context for a study of the politics of the Civil War era is in the political sphere. The scholarly works of James McPherson, W. R. Brock, John Cox and LaWanda Cox, Hans Trefousse, Michael Benedict, and Allan Bogue were reviewed in my introduction.

Jones writes that I show "little actual sociological or historical evidence that there was . . . a strong influence [of religion]" on political affairs during the war. I showed that religious and moral questions were brought forward more prominently during the war than any other question, but I only claimed that the Radical Republicans were significantly influenced by moral issues, not dominated by them. I wrote, "since human motivation is essentially subjective, the precise nature of the impetus for all radical action is difficult to determine," and church members made up less than one-fourth of the total population (pp. 4, 182).

Without any previous discussion of politics other than the operations of the Freedmens Bureau and without any documentation, Jones claimed as much political influence for clergy as I did, and he did not admit limiting factors as I did. In his 1991 review, Jones conceded that religious influence was only a "plausible" thesis. Yet he maintained in *Sectional Crisis* that "clergymen from the Methodist Episcopal

Church were on the front line in efforts to pass the Civil Rights bill . . . and continuously agitated for passage of laws granting suffrage to Negroes" (p. 293). "Also not to be forgotten is that during this period of considerable Methodist political influence, the Thirteenth, Fourteenth, and Fifteenth Amendments were adopted with the enthusiastic and concrete support of northern Methodism" (p. 299).

VICTOR B. HOWARD
Morehead State University

Donald G. Jones does not wish to reply.

THE EDITOR

TO THE EDITOR:

Obtuse and narrow reviews are not uncommon in our profession, but Bruce Frier's discussion of my recent book (*The Human Measure: Social Thought in the Western Legal Tradition*, AHR, 96 [December 1991]: 1503) is truly a message from Flatland. I have had many critical reviews and have never responded, but his is a case of such fundamental misunderstanding that I feel compelled to write in defense not merely of myself but of a whole dimension of historical inquiry.

Frier's review begins with a mistake more "astounding" than the one he attributes to me—that my book is about the "legal tradition" or Western jurisprudence; it is not, it is about the "Western social thought" figuring therein. Then he goes on to find me "wrong" about the trichotomy (persons/things/actions, or obligations). To Frier, the trichotomy is a trivial or technical legal matter restricted to Roman jurisprudence (a field in which he is indeed an expert); but to me it is a structural principle that has haunted and informed the human sciences and philosophy for two thousand and more years. Hard as it may be for Frier to perceive or to imagine, I am not writing Roman legal history. I am in no way concerned to divine Gaius's intention (certainly not "from a legal perspective"), to summarize the literal and legal meaning of the terms (which any legal dictionary can provide), to repeat what legal scholars say about it, or to address "modern lawyers," who are not famous for their interest in or anxieties about history.

"Gaius is describing 'all the law,' not the 'social field,'" Frier instructs; but my book is indeed concerned with the social field and in particular with tracing the fortunes of this structural principle in later writing about the human condition—the free or privileged individual distinguished from the world of *res* (public as well as private) and expressed by "action." I do not represent this trichotomy ("pedestrian," Frier calls it) as a paradigm of modern social thought but as part of the deep structure of social thinking (in contrast to natural science models, which are not person-centered)—"archeology," I would have called it if I had wanted to borrow theoretical luster or notoriety from Foucault. The analogy I

suggest is the trichotomous structure of Western languages (subject/object/predicate and variations), and my aspiration was to trace the legal homologue of this structure and its interpretations (Frier would say misinterpretations, but then that is what intellectual history is all about) *beyond* the legal technicalities rehearsed by Frier and through what I believe to be an interpretive community (and a continuous debate) that includes not only Grotius, Domat, Vico, and Montesquieu but also Hegel, Marx, Weber, and some of the founders of political economy, sociology, and anthropology.

Frier has nary a word to say about any of these themes beyond the Roman legal arena, nor does he seem to be even "dimly" aware of the conceptual or historical issues involved or indeed of his own consequent status as *iudex suspectus*. I appeal against this judgment *per imprudentiam*.

DONALD R. KELLEY
Rutgers University,
New Brunswick

BRUCE FRIER REPLIES:

Despite the intemperate tone of his letter, Donald Kelley raises two important questions about the kind of intellectual history he is writing. As he states, *The Human Measure* attempts to locate "the deep structure of social thinking" within the Western legal tradition. That structure is, so he thinks, a trichotomy: "persons/things/actions" or (using a grammatical analogy) "subject/object/predicate."

First, how does Kelley know that he is correct in identifying this particular trichotomy as "the deep structure"? In his book, Kelley argued that his views received direct textual support from a famous passage in which the Roman jurist Gaius divides Roman private law into three parts. However, as I showed in my review, Kelley's position is wholly untenable, since it rests on an egregious misinterpretation both of the passage itself and of the many later lawyers and legal philosophers who adopt or adapt it. Kelley has now evidently abandoned his earlier argument.

But Kelley's error has other implications. It is untrue that I regard Gaius's tripartite division of Roman law as "a trivial or technical matter restricted to Roman jurisprudence"; such a view would be ridiculous. Gaius's trichotomy obtained its huge influence for one major reason: it advances an "internal" perspective on legal norms, and accordingly it ignores and implicitly rejects any attempt to view law through a "social field" such as the one that Kelley attempts to construct. Gaius, like other Roman and later jurists, thus effectively brackets out of consideration the external "social field" in which legal norms operate. This means that the Gaius passage, far from supporting Kelley's thesis about a "deep structure of social thinking" in Western law and legal philosophy, actually militates against it.

Nonetheless, as Kelley now more correctly argues,

a "deep structure" does not inevitably require direct support in the literal meaning or intention of historical texts; that is to say, Kelley may indeed be justified in simply forgoing guidance and support from Gaius and his followers. The only question that we can then profitably ask about his proposed "deep structure" is this: despite its admittedly hypothetical character, does it still have genuine explanatory power in illuminating intellectual history?

Kelley obviously continues to believe that his "deep structure" does have such power. As I stated in my review, I myself have considerable doubts about whether Kelley's theory, even if it is construed most charitably to its author, is either coherent or interesting. Regrettably, the problem here is less with the theory itself than with Kelley's exposition of it. In particular, his prose style is so obscure and lyrical that I often find it impossible to make out exactly what he means or where he and his sources part company. Especially in regard to this latter question, problems such as his misinterpretation of Gaius necessarily become acute.

Still, I would not want to exclude the bare possibility that Kelley is on to something important. What is certain, in any case, is that *The Human Measure* has major defects and should not have been published in its present form; it does not succeed in clearly locating the deeper "structural principle" that Kelley claims is latent within the Western legal tradition. My fear is that this book's signal failure will bring the entire enterprise into disrepute.

Therefore, by way of antidote, let me give an exemplar of how scholars properly handle a historical topic of this sort: Maximilian Herberger's *Dogmatik: Zur Geschichte von Begriff und Methode in Medizin und Jurisprudenz* (1981).

BRUCE W. FRIER
University of Michigan,
Ann Arbor

ERRATUM

TO THE EDITOR:

I was delighted that James W. Reed cited my work on eugenics in his review of Angus McLaren's *Our Own Master Race: Eugenics in Canada, 1885–1945* (1990) [AHR, 97 (February 1992): 318] as among "recent notable works" on the history of eugenics. However, his citation (Ph.D. diss., Washington University, St. Louis, 1987) is incorrect. My dissertation is "A History of the American Eugenics Society" (University of Illinois, 1988). For those interested, it is currently being revised for publication by Indiana University Press.

BARRY MEHLER
Ferris State University

American Historical Association

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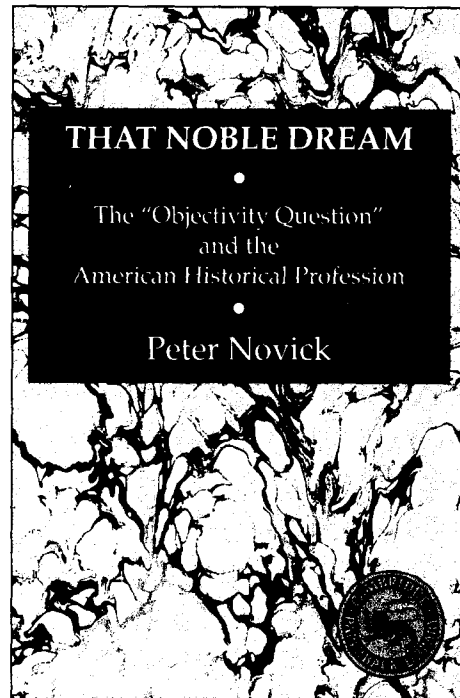
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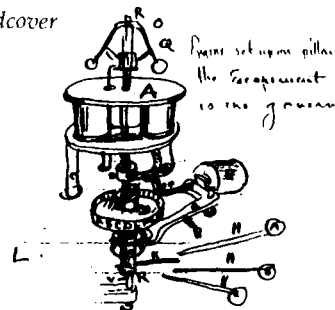
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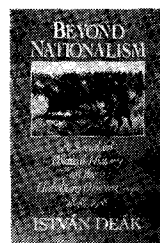
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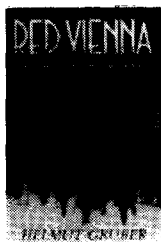
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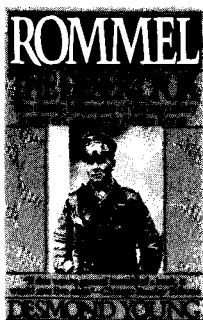
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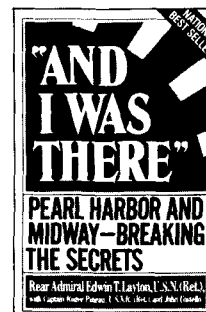
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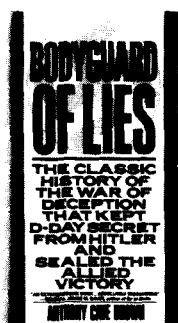
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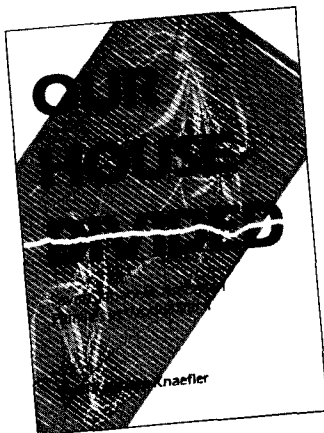
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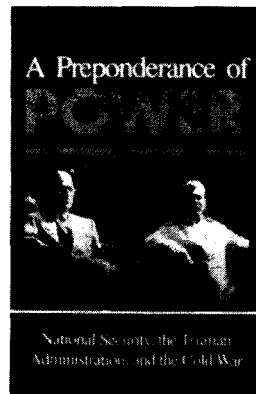


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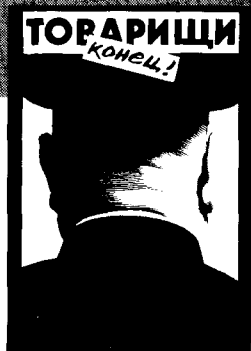
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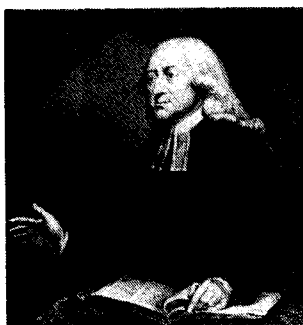
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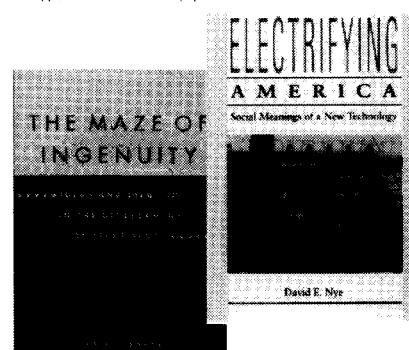
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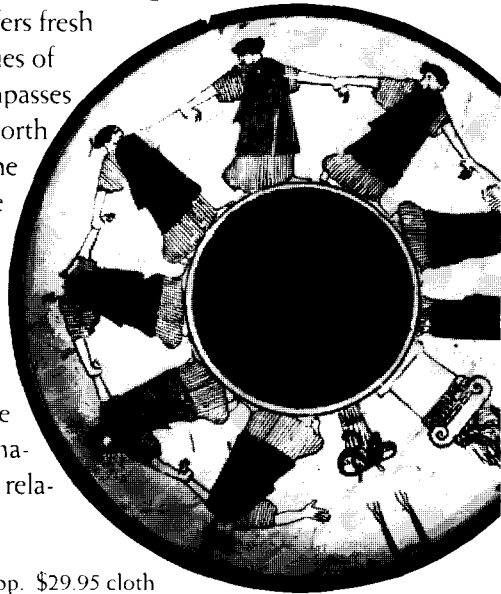
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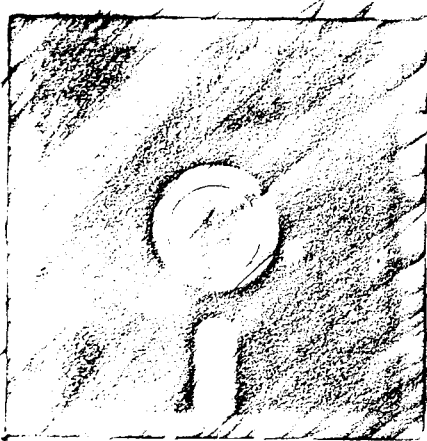
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